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THE PSYCHOLOGICAL REVIEW

HEREDITY, ENVIRONMENT, AND DEVELOPMENTAL PROCESS

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I. Evolution and Genetics

The assumption that race and individuality are corollaries of heredity, that they are largely biophysical phenomena, and that their interpretation depends on the principles and methods of biology is current among most biologists, many psychologists, and some sociologists.[1] The assumption that environment is causally related to individuality and race has also found its advocates among scientists.[2] Both concepts—environment and heredity—therefore stand in need of definition with especial reference to that which they are intended to interpret, viz., the developing organism. Such definition, to be valid, must derive from the analysis of the several varieties of evolution theory, the contributions of modern experimental genetics, and the findings of the social sciences.

The old theory of preformation in ontogeny assumed the individual to be complete in minutest detail in the protoplasm of the fertilized ovum. No such theory is entertained today. There is no preformed architecture of determinants within the parent nucleus which represents on a small scale the future structure of the organism. The extreme opposite of this theory—Lamarck's doctrine of environmental creationism—has also been called into question. Though this contained no teleological implications of the sort which made the first

[1] See Bateson (3), Davenport (24), Ewer (29), Hankins (34), Jenks (39), McDougall (59), Parshley (73), Pearl (74).

[2] See Bernard (7), Kantor (47), Loeb (55, 57), Packard (72), Watson (80).

theory narrow and unprofitable, yet it seemed naïve in its view that what is influential in development may be separated from what is developing and given a preferred place in the analysis.

In spite of its general crudity, Lamarckism prides itself on a good deal of empirical evidence adduced in its favor which, for many types of active adaptation, seems superior to the evidence supporting rival hypotheses (**28**, a). And, if we cannot agree with orthodox Lamarckism, neither can we accept the anti-Lamarckian views which base themselves on phylogenetic processes for which there is no adequate empirical evidence (**28**, b). Of course, Darwin assumed, very much as Lamarck had assumed, that variations originate in the body and are transferred to the germ cells. Weismann could not however, without resorting to an extraneous factor, explain how the germplasm comes to be the seat of somatic variations. His compromise, as is known, consisted in the hypothesis of parallel induction (**28**, c), in which he held that the gametes and the soma-cells are different from, and independent of, one another in spite of the fact that both are derived from the zygote cell. With the reservation that the gametes could never (as Darwin and Lamarck had assumed) come from the soma, he left the way open for changes to take place through a simultaneous involvement of both the soma and the germ cells. Thus Weismann's notion of parallel induction is an admission, in part, of Lamarck's contention.

The theory of parallel induction has not met with the general approval of biologists. There has been an increasing emphasis on the interdependence of germ cells and body cells and a vigorous insistence that both are derived from the same undifferentiated mass. The compromise view has thus resulted that the individual is compounded of variations or changes of two distinct kinds: one internal and the other external. The first kind of change has been assumed to originate in the germ-cells or gametes; the second to be produced by the action of external stimuli subsequently affecting the gametes (**23**, a). By postulating a constant intercommunication between the tissues of the organism and

their determinants in the germplasm, neo-Lamarckians have at the same time provided a mechanism of communication which Lamarck, Darwin, Weismann, and others sought in vain to supply. Unlike the fictitious gemmules, ids, idants, or constitutional units, the *hormones* treated more recently as agents of transmission (operating through blood, lymph, and interstices of tissues) have been given a meaning which the older imaginary carriers did not possess. In this way the neo-Lamarckians have sought to settle the question of external influence without resorting to an extra-experiential entelechy or the notion of pre-established harmony to explain how variations come to fit external conditions.

The avoidance of 'preformation' fallacies could not be credited to those who attacked the problem from the point of view of inner change. Still, the doctrine of Mendel, built on the thesis that organismic changes are gradual and inner and determined by a definite mathematical plan, has shown a tenacity which none other has had. It is only recently that the work in animal genetics and comparative embryology began to throw doubt on the literalness and exclusiveness of Mendel's hypothesis of inner change. Aside from the general question as to whether the doctrine applies alike to plants, and animals, and men (**8**a, **60**a), it appears certain that Mendelism has more to account for in the matter of dominance and recessiveness, the matter of unit characters, and the general matter of resemblance between parents and offspring.

It is the claim of Mendelists that variation arises by differentiation within the protoplasm, the ingredient characters of the plasm grouping themselves in pairs (allelomorphs) of which the dominant one suppresses the other known as the recessive character. It is always difficult and often impossible, however, to determine whether the transmission of a certain trait is explainable on the basis of dominance or of recessiveness, whether it is controlled by one or by several factors, and whether it is sex-linked or independent (**27**, a). Dominance itself is a relative term, for complete dominance is rare and its absence by no means uncommon (**68**, a). Furthermore, recessiveness is often an addition to the mor-

phology of the organism and dominance the absence of development of a certain quality or tissue (**68**, a). To say that the characters are latent in 'particulate fragments' in the chromatic substance of the nucleus (**62**), and as such are transmitted from parent to offspring, is to lose sight of the fact that only the presence, and not the absence, of a trait could be regarded as a 'particulate fragment' even from the Mendelians' point of view (**77**, a). What we know, in the last analysis, is that dominants and recessives do not submit to certain kinds of influences; but there is no way of determining how other influences might affect them, or whether such influences are not actually operating to produce the changes. We certainly do not know what effects dominant and recessive factors might have on one another if combined for a sufficient number of generations (**28**).

And contrary to what is commonly believed, the part played by the chromosomes is far from settled. Of the various characteristics, that of sex is claimed to be one which is most clearly determined by a 'nuclear configuration.' Yet even here it is admitted that there are cases in which sex appears to be determined by factors extraneous to the germplasm.[3] It is obvious now that a single gene may affect a number of characteristics and that a number of factors may be genetically related to the same character (**42**a, **43**a, **62**a, **63**a), since every gene affects the entire organism and every cell contains all the genes (**44**, a).

It is true that characters can be obtained in all associations within the limits of mathematical probability; but the fewer the number of allelomorphs, or points of difference, between the parents the greater the likelihood of resemblance between parent and offspring. Now since any two human beings differ in regard to hundreds of allelomorphs, the chances of resemblance between the traits of the parents and all the traits of the child are extremely remote: there is but one chance in many thousands.[4] As Jennings says:

[3] *Vide* the admission of so staunch a Mendelian as Conklin (**21**, p. 165); see also Carmichael (**10**, pp. 250–1).

[4] By a clever ruse Johannsen (**45**) attempted to get around this difficulty. Asserting that organisms that appear to be alike somatically are phenotypically

To be able to tell beforehand from the characteristics of the parents what will be the characteristics of the offspring has long been one of the dreams of science; but, to paraphrase the poet, 'now we know that we never shall know' how to do that in man; for the characteristics of the parent do not determine what combination of characters will appear in the offspring (**40**, p. 13).

To force a reconciliation of a supposed transmissibility of specific parental traits with the existence of a bewildering array of individual differences traceable evidently to extra-plasmic controls is to disregard the fundamental nature of development itself.[5]

The discovery that it is not characteristics in themselves that are transmitted in a particular way but only the differences between the two characteristics in the individuals mated (**44**, b), that a Mendelian formula for a characteristic in a certain set of cases may not be applicable to the same characteristic in another set of cases (**44**, c); and finally, that different characteristics show different laws of inheritance, because they depend on peculiarities arising from differences in methods of distribution (**44**, c), led to the conclusion that there are at least three types of inheritance in higher organisms: the typical Mendelian, depending on the autosomes (ordinary chromosomes), the sex-linked, depending on the x-chromosome, and that dependent on the y-chromosome (**44**, c). It seems probable however that even this classification, including the multiple-factor doctrine and the so-called theory of 'chromosomal aberrations,' does not exhaust all the possibilities of combinations in different parental stocks.

As an explanation of the growth of organisms in terms of inner change, the mutation hypothesis has been steadily gaining ground. Changes due to the chromosomal recombinations and segregations involving lethal factors, changes

(somatically) identical, but that organisms, whether alike somatically or not, that have the same determinants, are genotypically (germinally) identical, he thought that he had found added support for the Weismannian tradition. Johannsen's distinction between the phenotype and the genotype, growing as it did out of the need to explain the exceptions to Mendelian rules, seems a useless complication of the laws of 'heredity.' See Newman (**68**, p. 465) and Castle (**12**, p. 102).

[5] Compare Hankins (**33**, pref., viii) with Hankins (**34**, p. 219) to see how difficult it is to square the evidence with the theory generally held by Mendelians.

due to chromosomal aberrations, changes due to the modification of individual genes, and inductions—or changes resulting from permanent injuries to the whole germ cell—are now explained on the basis of mutation (**68**, b). The chief difference between the Mendelians and the mutationists is that the latter claim the effect of change to be evident in every part of the living structure, while the former would see it only in certain selected parts of the germ cell. For the Mendelians, then, the organism is a mosaic of separate elements corresponding to separate parts of the chromosomes and independent of all the rest; hence a change in any part will not affect the rest of the organism. The mutationists, on the contrary, see each part as inter-linked with each other—indeed, they see all the parts as merely quantitatively and not qualitatively different; hence, every change, no matter how minute, in any portion of the organism, must have its effect on the rest of the organism as well (**23**, b).

Where the Mendelians hold that characters arise as complete units, and not by gradual accumulation, and as such are transmitted (**2**, a), the mutationists speak of the emergence of new factors in the chromosomes, the loss of old ones, and the modification of existing factors. Because the mutationists do not stress the independent variability of each gene to the exclusion of the rest of the developing unit (**28**, d), their experiments constitute probably the most significant single contribution to biology in recent years. Operating with variations in temperature and chemical and nutritive conditions, and with x-rays applied to the chromosomes, they have obtained results which prove at once the tentative nature of the 'single factor' hypothesis and the limitations of Mendelism generally.[6] The fact that a large number of artificial mutations have not been transmitted does not alter the fundamental value of this research (**44**, d). The importance of it lies in the inevitableness of the conclusion that changes do not follow necessarily the so-called Mendelian ratios or even any of the other superpositions upon Mendelian

[6] For late researches see Muller and Dippel (**65**, pp. 85–122), Morgan et al. (**64**, pp. 217–239), and (**46**).

doctrine. Its further importance lies in the necessity of the assumption that there is something uniform and basic for all the organisms of a given species within the limits of which changes may arise.

For it is obvious that no matter how much individuals may differ in their genes, they all have had at the beginning something common to them all, which is probably represented in the once wholly neglected cytoplasm of the cell.[7] It is in the cytoplasm that the genes operate, changing it and being changed by it in a continual interaction which makes the parts of the organism thoroughly interdependent upon one another as well as upon their common base (**44**, e). Whatever reservations we may make with reference to the added interdependence of the cytoplasm and the genes upon extraplasmic factors (**15**, a), we must admit that even the most recent work on *maturation*, attempting to confine development to the intra-chromosomal changes, cannot displace the hypothesis of the interaction of factors in the inner environment.[8] By eliminating certain kinds of stimulation (through anesthesis) at a certain stage in development one may prove that development does not, at that stage, require those kinds of stimulation and can proceed independently of them, in the presence of other kinds of stimulation. But if maturation means that change of any sort may take place *without* stimulation it can hardly be said to be scientifically valid. Fortunately it is not always taken to mean that.[9]

Thus we arrive at the conclusion that biological studies have tended to emphasize three distinct types of early

[7] Jacques Loeb believed that the cytoplasm is of extremely great importance in that it carries the racial qualities of the individual, while the karyoplasm carries the specific, individual qualities. J. A. Thomson (**79**) has a similar point of view. Eldridge (**28**, p. 31) states that the germinal elements involved in the development of many body parts have been definitely localized (for some animals) in the cytoplasm of the egg cells. T. H. Morgan (**63**, p. 182), representing in this respect an older point of view, holds that the cytoplasm is in no way related to the racial germplasm, but that it exercises an important, if unknown, function in the development of the individual. Jennings (**44**, p. 77) agrees with Loeb, Thomson, and Eldridge.

[8] Carmichael (**11**). See also Coghill's theory of 'maturation versus exercise' (**18**).

[9] Says Wheeler: 'If maturation is a species of growth, one of its main conditions is stimulation' (**83**, p. 321).

organismal change: modification through environmental influence, combinations of differing parental stocks, and mutations.[10] If what has been called the 'inheritance of acquired characters,' taken in its orthodox form, seems crude and insufficient, Mendelism, when taken alone, also seems deficient, and just now exceedingly muddled. The certainty with which biparental inheritance was once promulgated has given way to cautious gropings along very different lines of research. The most secure contribution seems to have come from the representatives of mutation, for the evidence shows that, far from being direct and simple, the individual's 'inheritance' is multiple and indirect and ultimately traceable to a single source; and further, that the germ cells of an individual, in the last analysis, represent a set of factors different in many respects from that of any other individual in the immediate line of descent though similar to that of every member of his species.

2. Phylogenesis

The individual is a product of phylogeny and ontogeny. Phylogeny is defined as 'The history of the evolution of a species or a group' [11] or as 'The evolution of a race or a genetically related group of organisms (as a species, family, or order) in distinction from ontogeny or the development of the individual organism.' [12] Under phylogenesis we may therefore include (*a*) *selection* in the world of animal life and the segregation of the human species and (*b*) *differentiation* within the species and the segregation of races. Under ontogenesis we may include (*a*) *gestation, i.e.*, adjustment to the intrauterine environment, also known as prenatal development; (*b*) *parturition, i.e.*, adaptation to the environment of the so-called natal or delivery stage; and (*c*) *socialization, i.e.*, accommodation to the world of human culture, also known as post-natal existence. These we shall take up in the order given.

[10] These are named respectively: *die Paravariation, die Mixovariation, und die Idiovariation*, and are treated comprehensively by Erwin Baur (4, pp. 7–68).

[11] *New Standard Dictionary.*

[12] Webster's *New International Dictionary.*

The term *species* is by no means standardized. What some biologists call a species others call a genus, an order, or simply a variety. All biologists are agreed however that there are distinctive large groups of living organisms, possessing sufficiently similar traits of form, color, and structure to be thought of together, which on the whole are sufficiently dissimilar from other groups of organisms to be considered apart from them. Those who call these groups *species* are further agreed that the first test of a species is the relatively certain continuity of its general characteristics among its representatives from generation to generation.[13] The existence of one individual possessing a certain set of traits is taken as a reliable index of the pre-existence and the probable future existence of individuals having similar traits.[14] The numeric constancy of the chromosomes for each species, the interfertility within a species—and conversely, nonfertility or early death of offspring in crosses between individuals of varied species [15]—and also the differential tests of blood chemistry,[16] constitute even more definite proof of the differentiation of species.

Species are a product of environmental selection. Diverse environments affect development differently, but without environment development would be impossible. When some biologists declare that the environment merely permits and directs the evolution of species but 'does not cause it' (**17**, a), they fail to add, with equal certainty, what 'does cause it.' Surely one could not say that there is something within organisms equivalent to an ability to 'represent something

[13] The problem of the fixity of species has been changed to the problem as to why any species remains long constant. See Davenport (**25**, p. 311).

[14] To illustrate: If a starfish and a sea-urchin were placed in a finger bowl of sea water, under the same conditions, each would develop into a special type. If however two eggs of a starfish were placed one in plain sea water and another in sea water having double the amount of salt, the two starfishes would turn out to be entirely different. Davenport (**24**, p. 308).

[15] Jennings (**44**, ch. 12). For experiment on artificial interbreeding of organisms of the same genus but different species, see Newman (**67**, pp. 503–61).

[16] The work in this field was begun by Nuttall (**69**) and continued in a number of brilliant researches by Abderhalden, Gruber, Durham, Landsteiner and Miller, and others.

else even if that something has never been presented to that which represents it.' *Ex nihilo nihil fit.* All structure must be regarded as function structuralized in the process of adaptation to environmental situations.[17] Every species as a product of phylogeny represents the accretion of the structures by which it became differentiated from other species.[18] One definition of 'heredity' then is that it is a process by which the structures become materialized from a germplasm adjusted to certain conditions by a long line of ancestors (**15**, b).

Phylogenetic selection is a 'struggle' of the protoplasm for environmental adjustment, manifesting itself in that series of successive processes (competition, aggregation, inhibition, integration, and organic specialization) through which all species have passed. A number of environmental factors have coöperated to make these processes possible. The physico-chemical properties of water and carbonic acid, and of the earth's surface generally, assured the prevalent characteristics of all organic life (**37**). But it was the changes in season, food-supply, moisture, temperature, pressure, light, and general salinity (**12**, a), that have determined the variations among species of animal kind.[19] And even if all higher forms have attained a relative degree of independence of some of these factors, through the use of certain complex structures,[20] it is nevertheless obvious that the influence of these environmental factors has had a telling effect on the course of their evolution.

Changes in organisms have run closely parallel with changes in environmental factors in past geological ages. Natural selection has operated to eliminate rigidly specialized types and to preserve the more plastic constitutions (**68**, c).

[17] *Vide* White (**84**, p. 41). A happy illustration of the structuralization of function is offered by Colton (**19**, pp. 81–86).

[18] There is evidence, as implied in a previous note, that the differences between species inhere in the cytoplasm and depend on the degree of separation of the cytoplasmic parts. See Jennings (**44**, pp. 91–2).

[19] For experiment in gradual adaptation to temperature see Jennings (**41**, pp. 98–100); for adaptation to salinity see Wells (**82**, p. 157); for the effect of changes in the general environmental media on the modification of species see Baker (**1**, pp. 271–83).

[20] In human beings—the thyroid and the kidneys, for example.

That is probably why the human species has enjoyed a longevity not equalled by any other species of living things.[21] Unlike other representatives of the animal kingdom which have tended to stabilize certain types of activity, the human species has assured its survival by stabilizing lability (**22**). It is the plasticity of the human organism that has distinguished mankind from the other species. While stolid herbiverous monsters, large of body and small of brain 'gorged lazily on the abundant food at hand,' the fleet little carnivores came along to succeed them, but these in turn had to submit to the superior versatility and sensitivity of the anthropoids. What was evidently a quantitative difference began ultimately to appear like a qualitative difference in reaction to the existing relations (**22**, a). The recurrence of organism-environment relations made for continuity, and continuity made for uniformity of type.

Man is not however the apotheosis of the evolutionary process. He is merely the end product of a particular line of evolutionary changes. This argument properly interpreted means neither special creation nor fixity of species in a general sense. In Darwin's words, 'Man still bears in his bodily frame the indelible stamp of his lowly origin.' No one can therefore deny the reality of phylogeny in the history of the human species. Because of this men beget their own kind and not any other kind in the organic kingdom (**50**, a). Because of it too we witness a surprising similarity of human types everywhere, regardless of location. The adjustment of the phylogenetic protoplasm to a large variety of fluctuations in environmental media explains the ability of human beings to survive under the varied conditions of world geography; and once this adjustment is achieved, the uniformity of the phylogenetic protoplasm is reinforced and maintained by the stages[22] through which every member of the species passes in the course of individual growth.

[21] Smith (**78**, p. 4), for instance, defines man as 'the ultimate product of that line of ancestry which was never compelled to turn aside and adopt protective specializations either of structure or mode of life, which would be fatal to its plasticity and power of further development.'

[22] *Vide* the ontogenetic stages given later in this paper.

The fact of uniformity in the speciate endowment of the human individual was implied in Weismann's theory of the continuity of the germplasm and in Galton's theory of individual regression to a 'general average of the race.'[23] Both theories tend to show the existence of similarity in the phylogeny of individuals and human groups, though the first must be understood in the light of the qualifications already made, and the second remains to be tested on a wider scale. Still further evidence is found in the contention that even though *socially* the development of *Homo sapiens* has barely begun, the *biological* evolution of the species has practically reached its limit.[24] The implications of this evidence of the general similarity, though not identity, of human beings [25] raise the question as to the meaning of the taxonomic divisions of the species (races) accredited with special sets of transmissible qualities and, too, as to the justifiability of the assertion that personality types may be differentiated on the basis of pre-existing phylogenetic factors.

The differentiation of the species into racial groups has been the subject of much controversy among biologists and social scientists. The differences between the races of one species are far less definite than the differences between any two species. To say that an individual belongs to a given species is to say that neither he nor his offspring could belong to any other species, since the differences in chromosomal quality and quantity forever preclude interfertility. To say that an individual belongs to a given racial branch of a species (subspecies?) is to say that he possesses certain similarities to other individuals of the species and that he can interbreed with individuals of other races of the same species. Racial

[23] For a statement of these theories see Thomson (79) or any standard work.

[24] See evidence submitted by Conklin (20, pp. 59–70), Boas (9), and Osborn (70 and 71).

[25] Hankins (36, p. 106) assumes that the existing races of humanity do not all belong to the same species and refers to Darwin for authority, though Darwin's insistence was that the various races are subspecies of one and the same species. See Darwin (26, last chapter of pt. 1); also Haeckel et al. (32, pp. 127–29). Reuter (76, p. 708) musters considerable evidence to prove the correctness of such an assumption as is here made by the writer. Jennings too (44, p. 278) holds that there are no incompatible chromosomes in the different races of man.

differences in blood chemistry and in certain structural peculiarities (**51**) are inseparably connected with the characteristic topography, altitude, climate and food supply [26] which have for many years manipulated the protoplasm of the species and enforced its adjustment in diverse regions. The phylogeny of the species is thus to the phylogeny of any of its races as racial phylogeny is to ontogeny. Speciate phylogeny and racial phylogeny have however this in common with each other and with ontogeny in general: they depend on a relationship between the germplasm and certain environmental factors. The difference is merely one of *degree* of standardization and *degree* of variation in the existing relationships.

The dissimilarities found in the individuals of any race are even more notorious, if possible, than the differences between races. These cannot be explained except through the rôle of environmental factors in the consummation of the individual (**25**, a). It is now evident that no individual represents even in regard to his anatomical and physiological characteristics all the potentialities of his protoplasm; since only the potentialities suited to the environment can be realized in the lifetime of an individual.[27] This means that the development of the individual has indeed an 'hereditary' (standardized adjustment) basis, but that under changed conditions the 'basis' may prove altogether different and characteristics may then become established which would not otherwise make their appearance (**43**, b). This is true because the potentialities of the protoplasm belong evidently not to the individual as an individual but to the individual as a member of his species and his race (**8**b, **14**). The range of any man's heredity is potentially as wide as that of *Homo sapiens*, but the particular development and subsequent nature of the indi-

[26] The standardization of the environment is evidenced in the dietaries of the various peoples. *Vide* Krout (**51**).

[27] August Weismann's earlier supposition was that all the characters are carried by each of the chromosomes; in fact, he had assumed the existence of alternative sets of determiners in the germplasm, depending on one or another set of environmental conditions to call them into being. See Castle (**12**, p. 52). In bees, for example, the same egg may produce a queen or a worker, the result in either case depending on the amount and quality of food (*Ibid.*, p. 54).

vidual depend on the range of the developmental processes which evoke, inhibit, or fail to affect his 'hereditary' potentialities.

Within the limits of the groundwork perfected by the phylogenetic environment and transmitted as the 'germoplasmic potential' of the human species, the possibilities of variation in the somatoplasm of individuals are limited apparently only by the conditions of development. Thus the reason for the existence of unlikenesses in individuals cannot be found in phylogeny as such. The reasons must be sought in the ontogeny of the individual—that is, in the processes of individual development following conception. It is here, in the gradual interaction of cytoplasm and nucleus, that the individual comes into being—from the protoplasm but not through it alone; in the united germ-cells, yet not without a variety of pressures operating in the relationship between these cells and their general environment.

3. Ontogenesis

Ontogeny is not an epitome of phylogeny, for the development of the individual is not a recapitulation, in any reasonable sense, of the stages through which the species passes in the course of its development.[28] Ontogeny consists of a seriated system of relationships by which the individual gradually comes into the possession of some share of his speciate endowment. On the basis of the factors which operate to change the successive relations into which the organism enters, ontogeny may be conveniently subdivided into four stages.[29]

In the first stage, the *prenatal* (10, a), immediately following fertilization, such factors as barometric and gravitational pressures, electrical conditions, radiations,[30] temperature and

[28] For a contrary point of view see Davenport (25).

[29] Compare other classifications, such as are given in Bernard (6), Frank (30), Freud (31, pp. 285*ff.*), Jeliffe (38, pp. 225*ff.*), and Weiss (81).

[30] From the problem of genic modification through X-rays the question arose as to how mutations come about in normal life (66, pp. 84–7). The answer ventured is particularly applicable to this discussion of the factors in the prenatal stage. It was that the waves of short wave-length coming from the radioactive substances of the earth's crust or from the depths of space, produce a continuous effect on living organisms.

light conditions,[31] impacts, catalytic agents (enzymes), food elements (vitamins), and hormones control the processes of growth. The control of the oxygen supply,[32] the diffusion of the elements of foodstuffs (**77**, b) and intercellular competition for nutrients and position determine the fundamental gradients of symmetry and polarity (**13–16**) and shape the grosser body-orientation mechanisms. These, incidentally, may be instrumental in establishing predispositions to disease —through the interference of toxic products with gestation [33] —and many other now doubtful peculiarities of structure.

The natal stage is the transitional stage connecting the embryonic with the extra-uterine period of life. In this stage specific conditions accompanying delivery control the organism of the individual. Here belong possible traumas, infections, and the shock of birth itself with all its implications.[34] The endocrine balance and temperament of an individual may be profoundly affected in this period.

The last of these stages, the *post-natal*, may be divided into the *socio-physiological*, or pre-speech, stage and the *psycho-social*, or language habit, stage. The first occupies roughly the two years succeeding birth. In this stage, through the stimulation and inhibition of the basic physiological functions in the parent-child relationship, the child fixates his responses to inner pressures. These early effects of social experience become indelibly impressed upon the individual. There is some evidence for the contention that the reaction-patterns of

[31] For abnormal fluctuations in these factors, producing monstrosities, see Mitchell (**61**) and Jennings (**43**, pp. 43–5).

[32] J. Loeb (**55**, p. 25) has long ago shown that when freshly fertilized animal eggs are deprived of oxygen no nuclear division takes place, and this has been confirmed in subsequent investigations. P. Kammerer, for example, has proved that pigment formation is an oxidation process which in animals does not take place in the absence of light, though Wright and others attributed it to the enzyme element in the reaction (**12**, pp. 39 and 177). Compare this with the question of dominance and recessiveness —of eye-color, for example—and with the problem of maturation referred to previously.

[33] Compare with theories of predisposition to disease in Baur et al. (**4**) and Jennings (**44**, pp. 147–9).

[34] For the relation between birth and fear see Freud (**31**, p. 343), Berenfeld (**5**, pp. 214–226), and Rank (**75**). Like other psychoanalytic generalizations, this may be somewhat exaggerated, but it does not appear to be altogether without foundation.

later life hark back to the stimulus-situations met by the child and the responses he made to them in this stage of existence (**52**, a).

The last of the developmental periods—a period which is temporally the most extensive—begins when the medium of communication becomes primarily verbal and symbolic and to a less extent gestural and physiological. In this, the psycho-social stage, the individual learns consciously to participate in the life about him, acquires some degree of insight into the thoughts and motives of his fellows, conceives his group rôles, and achieves that integration of personal behavior-patterns which is known as character.

The life story of the individual is the story of a gradual ascent from an undiversified phylogenetic level (represented by the zygote) on through the pre-natal, natal, and post-natal levels. The pre-natal level, in which the racial and individual structures are selected from a totality of potential (speciate) structures, is obviously important. More important than that however is the natal stage in which every person passes through the first crisis of his life. Still more important is the socio-physiological stage in which the basic impulses become defined after a fashion, and the foundation of the later reaction-patterns of the individual is laid. But the psycho-social development surpasses, in degree of importance to adult behavior, both the phylogenetic and the early ontogenetic history of the person. This is not due to the fact that it is the last of the several stages of ontogeny, but rather to the length of time that the individual spends under its influence. Obviously the influence of any period is reduced by the fact that the importance of a situation is inversely in proportion to the lateness of the stage at which the situation operates. The psycho-social stage however, in a real sense, antedates the other stages. The state of health and the habits of the mother during pregnancy, the exigencies of delivery, and the question of infant care all go back to this environment. It is in this sense that the individual is said to be born into a pre-existing world.

In spite of the obvious effect of the psycho-social stage of

existence, representatives of the socio-biological movement have held with variations of emphasis that 'our original traits' not only do not submit to environmental conditioning but instead tend themselves to condition the environmental setting of the organism. Recent literature contains the argument that there is a vital distinction between 'organic plasticity,' or the differences in original traits due to varying environmental conditions, and 'organic responsiveness,' or the tendency of the organism to determine whether or not the individual will respond to a certain situation. On the basis of the second of these terms the claim is made that the inborn 'internal energies,' 'predispositions,' 'aptitudes,' and similar tendencies select the kind of influences in the environment that appear suitable to the individual's inherent nature (**35**).

As long as there is a possibility of explaining selective behavior without recourse to special fixation in phylogeny, a claim such as this is not justified (**53**). It is not only conceivable but probable that early ontogenetic conditioning results in patterns of response which act selectively with reference to all later patterns. Take, for example, Prospero's characterization of Caliban as 'a devil, a born devil, on whose nature nurture will never stick.' However popular the theoretical implications of this statement might have been in Shakespeare's day, or may still be in certain quarters, they are hardly unavoidable today. For it is not necessary to speak of the 'uncultured individual' as without culture in that his especial heredity 'chose' not to be cultured. Because of the control exercised by antecedent over subsequent patterns,[35] all that we can legitimately say is that such an

[35] This assumption seems to be in keeping with W. Köhler's hypothesis of 'non-process condition' in which he holds that every change effected by a stimulus is the result of a mutual adjustment between itself and a preëxisting system of stresses (**49** and **83**). Child (**16**, p. 30) holds that 'The record of past behavior becomes a fundamental factor in determining the character of all behavior and may even persist through cell-division and various reproductive processes and so become hereditary, but such inheritance is obviously not Lamarckian inheritance of acquired characters.' *Cf.* Jennings (**44**, pp. 149–51) referring to 'diversities resulting from past environments.'

individual has merely been 'cultured in the "wrong" way'—that is, literally, not 'in our way.'

The priority right that patterns exercise over one another throws a flood of light on the differential interpretation of defectiveness in individuals and backwardness in races. Neither the latter nor the former may be said to have a *different* hereditary equipment in the sense here used, but instead to represent merely a *less complete* expression of it. A defective individual or a preliterate group, drawing on the common phylogenetic fund, goes through the stages of development more slowly, every defect or instance of backwardness being traceable to factors which have failed to affect or else have restrained the growth process.[36] Such an individual or group may never reach, or may not profit by, a given period of ontogeny because of the controls operating in one of the formative stages. The time of retardation, *i.e.*, the successive changes causally related to the situations in the stage in which development was arrested, may determine the specific nature of the defect acquired;[37] and the subsequent employment on a higher level of patterns of response acquired on a lower level may be causally related to the general growth of the individual.[38] In other words, a person whose growth was retarded in the pre-natal stage may be born feeble-minded and thus utterly incapable of profiting by the advantages of a high-grade post-natal environment, and one whose earlier development has proved relatively advantageous may be hopelessly retarded in a backward post-natal milieu.

The analysis of the developmental process into stages gives

[36] Jennings (44, p. 8) represents the orthodox Mendelian view when he declares that feeblemindedness is due to the 'failure of a gene to lay a proper foundation for the brain.' He admits elsewhere that it is not necessarily transmitted from parent to offspring (*Ibid.*, pp. 16–20).

[37] For a technical treatment of the retardation influences in growth, see Robertson (77, ch. 7).

[38] From this point of view *all* patterns are potential. Being priormost in the genesis of the person, phylogeny may have a selective influence upon the later ontogenetic patterns, but only in the sense that the tremendous multiplicity of the potentia patterns of the germplasm must be evoked (employed) in a higher ontogenetic stage in order to become actual.

us a rough schema of the natural history of every human individual. The developmental stages represent different types of relations into which the organism enters at different periods of its existence. These relations may be said to be standardized insofar as the behavior that occurs within them is conditioned upon the preceding situations and itself exercises a selective influence on all succeeding situations. Where a certain ratio is discovered in the morphology or behavior of several generations of individuals [39] the ratio is indicative of the continuity of a certain relationship between factors, and not of the fixedness—in a static sense—of the morphological or behavioral characteristics involved.[40] From this standpoint it is not beneficial to describe a characteristic of any kind as determined by heredity or by environment, for all characteristics develop within a certain range of standardized relations and with reference to certain types of pre-existing situations.

To say that certain environments are limited to given functions or that given organisms are standardized for certain types of activities is thus neither helpful nor proper. Both environment and organism are parts of total situations, and both are relative to those situations. Organisms standardized for certain functions cannot exist unless the conditions favor those functions; and the environmental conditions are eternally dependent on what a previous interaction, present at another stage, has prepared for utilization in a stage to follow.

4. Conclusion

The laws of heredity and what biologists call the mechanisms of inheritance overlap in more than one respect. It is futile therefore first to dogmatize on the nature of inheritance and then to search for the means whereby heredity becomes

[39] *Cf.* this viewpoint with the theory of Mendelism.

[40] Jennings (43, p. 45) admits that this may be the case, but he supposes that germinal factors may follow a similar ratio somewhat independently. Child (13, p. 41) states that, once a gradient is established, it may 'persist through the processes of cell-division or other forms of reproduction so that the unity and order of the new individual represent the unity and order of the parent or a part of it'; which may be interpreted as confirmatory of the thesis here advanced.

possible. It is also futile to attribute to one factor what is logically and actually a function of many factors operating in a closely-knit relationship, for to do so is to overlook the dynamics of life itself. It is unnecessary, though not uncommon, to make processes 'more concrete,' *i.e.*, to reify abstractions, thus committing an error common to some extent among all scientists but especially pernicious as a practice in the field of biology.[41] Beset with these limitations however, much biological theory—whether in the hands of its humblest or in those of its most illustrious representatives—appears to be based on pre-existing biases manifesting a powerful 'strain toward consistency.' As such it is a tissue of loosely organized strands of knowledge which is by no means ready for application to matters of social policy and polity.[42] Nor can it be until the antithes is between heredity and environment is done away with and the realization dawns upon biologists that heredity and environment, instead of being opposed to each other, are both merged in the concept of *developmental process*.

The organism as it is known at any time cannot be separated from its surroundings (**54**). It develops with and within them. The intraindividual environment and the influences which the substances within the nucleus exert upon one another and upon the cytoplasm must be recognized; for every part of the cell is an organ of correlation and 'produces something which plays a rôle in making other parts what they are' (**13**). The continuous interaction of physical and

[41] Jennings (**44**, p. 24) falls into the error of orthodoxy when he speaks of the inheritance of abstract traits such as industry, ambition, intelligence, natural quickness, personality, etc. as if these could be represented by the individual, or even collectivities of, genes as such. In view of his generally healthy outlook, it seems strange that he should commit so obvious an error. For the pioneer discussion of this error see Bernard (**7**, chs. 11–17).

[42] The work of Mendelian eugenists is an illustration in point. Their error consists not merely in their hasty adoption of an incompletely proved theory but, to an even larger degree, in the supposition that there is sufficient biological knowledge available at present as to what mental and moral qualities, and what types of cultural existence, are most desirable, and in the long run will prove most beneficial. Even from the standpoint of biology it is by no means certain that there is a cumulative effect of continued selection within a pure strain, granted that such is obtainable. On this point see Love (**58**).

chemical factors upon the fertilized egg, during the prenatal and the natal stages of existence, cannot be neglected.[43] And, surely in the case of human individuals, sight cannot be lost of the far-reaching importance, in the post-natal stage, of the interaction of the organism as a whole upon other organisms—an interaction which is contingent upon those definitive patterns (relationships) giving individual activity its meaning and direction.

The separation of *heredity* and *environment* is thus an error for which there is no excuse. We cannot conceive of them as apart from one another for the reason that they are parts of the selfsame process. The developing organism is a function of the *relation* between the protoplasm and its environment, and not strictly speaking of the one or of the other. To realize this is to ask: Is this relation of a changeable or of an unchangeable nature? Ordinary observation supports the view that the relation *is* changing constantly and naturally. The question then is not, 'Can the developmental process change?' for to deny this clearly would involve a contradiction in terms; but 'How can the relations which exist be changed so that the developmental process can be something different from what it is?'

Those who cannot see heredity and environment together have often asked whether the developmental process may be changed most expeditiously by changing the genes or by changing the environment in which they function. This question seems to be of little consequence (**44**, f). The importance of the change, when produced, lies in the altered relations secured as a result. Whether the developmental process is attacked by one means or by another is not anywhere as important as the fact that what is changed is the developmental process; for changing the genes in relation to the factors to which they stand related or changing the factors which stand related to the genes is not a matter of *qualitative* difference at any rate (**44**, g).

Biologists who standardize environmental conditions in an

[43] Keibel and Mall (**48**, ch. 20), in their discussion of the interdependence of various developmental processes, offer a good account on this point.

attempt to discover germinal changes as something separable from environmental influence are engaged in an elusive undertaking. They may create extremely varied ontogenetic conditions and so make some vital discoveries with reference to the effect of pre-natal conditions on the individual, particularly in terms of degrees of standardization which they permit. They can go further and study the influences inhering in the natal process and the types of standardization which it permits. But they cannot isolate the protoplasm from the conditions under which it grows and develops. It is only through a study of processes as changes in interrelated factors on the pre-natal and on the natal levels that the biologist can bring the assistance of his technique to the study of the evolution of human races, and incidentally to social policy and polity.

The biologist and the social scientist cannot compete. The biologist interested in the analysis of interactions of physico-chemical substances and of 'packets of genes' cannot hope to include all the relations into which the organism enters. The social scientist on the other hand does not study any relations but those between human beings—whether conscious or not—and the products resulting from social interaction. The human personality, as an aspect of individuality; and group culture, as an aspect of racial existence, can no more be in the field of the biologist than cell-processes can be in the field of the social scientist.

Bibliography

1. Baker, F. C. The influence of a changed environment in the formation of new species and varieties, *Ecology*, 1928, 9.
2. Bateson, W. Mendel's principles of heredity, Cambridge (Eng.), 1909, (a) 288.
3. ——. Biological fact and the structure of society, Oxford, 1912.
4. Baur, E., Fischer, E., and Lenz, F. Menschliche Erblichkeitslehre und Rassenhygiene, Munich, Lehmann, 1923.
5. Berenfeld, S. The psychology of the infant, N. Y., Brentano, 1930.
6. Bernard, L. L. The significance of environment as a social factor, *Proc. Amer. Sociol. Soc.*, 1921, 16.
7. ——. Instinct: A study in social psychology, N. Y., Holt, 1924.
8. ——. An introduction to social psychology, N. Y., Holt, 1926, (a) 61, (b) 101.
9. Boas, F. Growth and development, bodily and mental, as determined by heredity and by social environment, in The child, the clinic, and the court (a Symposium), N. Y., New Republic, 1925.

10. CARMICHAEL, L. Heredity and environment: are they antithetical? *J. Abn. & Soc. Psychol.*, 1925, **20**, (a) 256.
11. ——. Development of behavior in vertebrates experimentally removed from the influence of external stimulation, PSYCHOL. REV., 1926–1928, **33**, **34**, **35**.
12. CASTLE, W. E. Genetics and eugenics, Cambridge, Harvard Univ. Press, 1924, (a) 32–8.
13. CHILD, C. M. Individuality in organisms, Chicago, Univ. of Chicago Press, 1921.
14. ——. Physiological foundations of behavior, N. Y., Holt, 1924.
15. ——. The beginnings of unity and order in living things, in E. S. Dummer (ed.), The unconscious, N. Y., Knopf, 1927, (a) 34; (b) 33.
16. ——. Biological foundations of social integration, *Proc. Amer. Sociol. Soc.*, 1928, **22**.
17. COE, W. R. Organic adaptation to environment, New Haven, Yale Univ. Press 1924, (a) 147.
18. COGHILL, G. E. Anatomy and the problem of behavior, Cambridge (Eng.), 1929.
19. COLTON, H. S. The biped habit, *Scient. Mo.* 1930, **31**.
20. CONKLIN, E. G. Direction of human evolution, N. Y., Scribner, 1922.
21. ——. Heredity and environment in the development of men (rev.), Princeton, Princeton Univ. Press, 1923.
22. CRILE, G. W. Man—an adaptive mechanism, N. Y., Macmillan, 1916, (a) 17–39.
23. CUNNINGHAM, J. T. Hormones and heredity, London, 1921, (a) 37; (b) 215.
24. DAVENPORT, C. B. Heredity in relation to eugenics, N. Y., Holt, 1911.
25. ——. Light thrown by evolution on genetics and development, *Scient. Mo.*, 1930, **31**, (a) 307*ff*.
26. DARWIN, C. Descent of man, 2nd (pop) ed., 1901.
27. EAST, C. M. AND JONES, D. F. Inbreeding and outbreeding, Phila., Lippincott, 1919, (a) 229.
28. ELDRIDGE, S. The organization of life, N. Y., Crowell, 1925. (a) 109*ff*.; (b) 110; (c) 34–50 and ch. 3; (d) 325–8.
29. EWER, B. C. Social psychology, N. Y., Macmillan, 1929.
30. FRANK, L. K. Physiological tensions and social structure, *Proc. Amer. Sociol. Soc.*, 1928, **22**.
31. FREUD, S. General introduction to psychoanalysis, N. Y., Boni and Liveright, 1920.
32. HAECKEL, E., THOMSON, J. A., WEISMANN, A., ET AL. Evolution in modern thought, N. Y., Boni and Liveright (n.d.).
33. HANKINS, F. H. Racial basis of civilization, N. Y., Knopf, 1926.
34. ——. An introduction to the study of society, N. Y., Macmillan, 1928.
35. ——. Organic plasticity versus organic responsiveness in the development of personality, *Proc. Amer. Sociol. Soc.*, 1928, **22**.
36. ——. Racial relationships in international harmony, *World Unity*, 1929, **4**.
37. HENDERSON, L. J. The fitness of the environment, N. Y., Macmillan, 1913.
38. JELIFFE, E. S. Paleopsychology, mental and nervous dis. series, No. 41, Studies in Psychiatry II, Washington, 1925.
39. JENKS, A. E. Hereditary human groups in their relation to distinctive cultures, *Proc. Amer. Sociol. Soc.*, 1921, **16**.
40. JENNINGS, H. S., WATSON, J. B., MEYER, A., AND THOMAS, W. I. Suggestions of modern science concerning education, N. Y., Macmillan, 1917.

41. JENNINGS, H. S. Life and death, heredity and evolution, in unicellular organisms, Boston, Badger, 1920.
42. ——. Heredity and environment, *Scient. Mo.*, Sept., 1924, (a) 228–30.
43. ——. Prometheus, or biology in the advancement of man, N. Y., Dutton, 1925. (a) 21–2, 38; (b) 39.
44. ——. Biological basis of human life, N. Y., Norton, 1930. (a) 16, 99, 107, 197; (b) 69; (c) 65; (d) 319; (e) 78; (f) 215*ff.*; (g) 134.
45. JOHANNSEN, W. The genotype conception of heredity, *Amer. Natur.*, 1911, 95.
46. ——. *J. Hered.*, June, 1929 (entire issue).
47. KANTOR, J. R. Principles of psychology, 2 vols., N. Y., Knopf, 1924–1926.
48. KEIBEL, F. AND MALL, F. P. Manual of embryology, 2 vols., Phila., Lippincott, 1910–1912.
49. KÖHLER, W. Gestalt psychology, N. Y., Liveright, 1929.
50. KROEBER, A. L. Anthropology, N. Y., Harcourt, 1923, (a) ch. 2.
51. KROUT, M. H. Race and culture: A study in mobility, segregation, and selection, *Amer. J. of Sociol.*, 1931, 36.
52. ——. The psychology of children's lies, *J. Abn. & Soc. Psychol.*, April, 1931, (a) sec. 2, 3.
53. ——. Culture and culture change: A contribution to social psychology, *Amer. J. Sociol.*, 1931, 37.
54. ——. Personality-testing in the light of the situational approach, *Amer. J. Psychiat.*, March, 1931.
55. LOEB, J. Artificial parthenogenesis and fertilization, Chicago, Univ. of Chicago Press, 1895.
56. ——. The mechanistic conception of life, Chicago, Univ. of Chicago Press, 1912.
57. ——. Forced movements, tropisms, and animal conduct, Phila., Lippincott, 1918.
58. LOVE, H. H. Are fluctuations inherited? *Amer. Natur.*, 1910, 94.
59. MCDOUGALL, W. Outline of psychology, N. Y., Scribner, 1923.
60. MEYERSON, A. Psychiatric family studies, *Amer. J. Insan.*, 1917, 123, (a) 360.
61. MITCHELL, P. H. Textbook of general physiology for colleges, N. Y., McGraw-Hill, 1923.
62. MORGAN, T. H. Physical basis of heredity, Phila., Lippincott, 1919, (a) 240.
63. ——. Mechanism of mendelian heredity, N. Y., Holt, 1922, (a) 264.
64. MORGAN, T. H., BRIDGES, C. B., AND STURTEVANT, A. H. Genetics of drosophila, *Bibliographia Genetica*, 1925, 11.
65. MULLER, H. J. AND DIPPEL, A. C. Chromosome-breakage by x-rays and the production of eggs from genetically male tissue in drosophila, *Brit. J. Exper. Biol.*, 1926, 3.
66. MULLER, H. J. Artificial transmutation of the gene, *Science*, 1927, 66.
67. NEWMAN, H. H. The process of heredity as exhibited by the development of fundulus hybrids, *J. Exper. Zoöl.*, 1908, 5.
68. ——. Evolution, genetics, and eugenics, Chicago, Univ. of Chicago Press, 1925. (a) 414; (b) 469*ff.*; (c) 526.
69. NUTTALL, G. H. F. Blood immunity and blood relationship, Cambridge (Eng.), 1904.
70. OSBORN, H. E. Men of the old stone age, N. Y., Scribner, 1916.
71. ——. The proposed suppression of the teaching of evolution, *Science*, N.S., 1922, 4.

72. PACKARD, A. S. Lamarck, the founder of evolution, his life and work, London, 1901.
73. PARSHLEY, H. M. Biology, in H. E. Barnes (ed.), The history and prospects of the social sciences, N. Y., Knopf, 1925.
74. PEARL, R. Studies in human biology, Baltimore, Williams and Wilkins, 1924.
75. RANK, O. Trauma of birth, N. Y., Harcourt, 1930.
76. REUTER, E. B. The relation of biology to sociology, *Amer. J. Sociol.*, 1927, 33.
77. ROBERTSON, T. B. The chemical basis of growth and senescence, Phila., Lippincott, 1923. (a) 316–7; (b) 11*ff*.
78. SMITH, G. H. The evolution of intelligence and the thraldom of catch-phrases, in C. M. Campbell et al., Problems of personality: studies in honor of Morton Prince, N. Y., Harcourt, 1925.
79. THOMSON, A. J. Heredity, N. Y., Putnam, 1926.
80. WATSON, J. B. Behaviorism: Lectures in print, N. Y., Norton, 1924–1925.
81. WEISS, A. P. The measurement of infant behavior, *Psychol. Rev.*, 1929, 36.
82. WELLS, W. R. The meaning of 'inherited' and 'acquired' in reference to instinct, *J. Abn. & Soc. Psychol.*, 1922, 17.
83. WHEELER, R. H. The science of psychology, N. Y., Crowell, 1929.
84. WHITE, W. A. The meaning of disease, Baltimore, Williams and Wilkins, 1926.

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THE LAWS OF ASSOCIATION

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The terms 'association' and the 'laws of association' have lost caste in recent years. One almost feels constrained to apologize for referring to or using them. The term 'conditioning' is the present fad. Conditioning refers to no new phenomenon, however, since it is but a synonym for 'association.' The introduction of the new term probably did deceive a few into thinking that they were dealing with a new phenomenon; but the great majority of psychologists were keenly aware that the only innovation was a terminological one.

The introduction of the new term did serve a useful purpose in emphasizing certain aspects of the general phenomenon. The term 'conditioning' was employed in dealing with the formation of sensori-motor connections, while 'association' was first used in dealing with ideational connections. However, we may call attention to the fact that Hartley early extended the concept to the realm of motor responses—a fact that has frequently been noted in times past. Hartley's attitude exerted but little influence upon the later associationists, but the modern psychologists—especially the Americans—did systematically employ the term in its broadest connotation. For example, Thorndike in his *Elements of Psychology* (first published in 1905) utilized the concept in dealing with the simplest stages of habit formation, and the term had also been so employed in his earlier experimental studies on Animal Intelligence. In fact, all comparative psychologists consistently employed the term in this way before the time of the Russian importation.

Outside of terminology, Pavlov's experiments, in particular, made a noteworthy contribution, in showing that the concept of 'conditioning' or 'association' is also applicable

to the field of the somatic or vegetative activities, and in emphasizing the intimate functional relationship that obtains between mental events and the vegetative processes.

I am not interested in terminological disputes. This paper is concerned with certain aspects of the phenomenon which these terms connote. It is wholly immaterial to me whether one speaks of the phenomenon of 'association' or 'conditioning,' of the 'laws of association' or the 'laws of conditioning'; the actual phenomena of which we are speaking are the same, and the only difference is that of the language we employ in dealing with these phenomena. In this paper I shall arbitrarily employ the terminology of 'association,' but those who choose can translate these terms into the language of 'conditioning' if they so desire.

The general phenomenon to which these terms refer is the formation and functioning of all sorts of stimulus and response connections. I propose to discuss the descriptive and explanatory technique that has been developed in dealing with this phenomenon, and which is usually referred to as the 'Laws of Association.'

In passing, it may be well to note that it has been the Associationists who have attempted to work out such a technique, while those who have used the language of 'conditioning' have been somewhat remiss in this respect. Some have utilized the secondary laws of Frequency and Recency, and have casually referred to the conditions under which responses become conditioned to new stimulations as *simultaneity* rather than *contiguity*. But in the main there has been no systematic attempt to work out a comprehensive conceptual technique of this sort.

The laws of association are usually divided into the two classes of primary and secondary. There have been individual differences as to the number of laws listed; but as to the secondary laws the tendency has been to follow James rather than Brown. The Aristotelian tradition has been dominant in respect to the primary laws, with the added conception that all of these laws can be reduced to the fundamental law of *contiguity*.

This paper advances the proposition that the number of laws needs to be considerably increased and also reclassified.

The first distinction proposed is one between the descriptive laws and the explanatory laws. For example, the conventional primary laws of similarity and contrast are descriptive laws, while contiguity is explanatory in its import. The law of similarity states that the thought of an object tends to arouse the thought of a similar object. This statement is based upon the observed fact that this type of sequence has occurred with a sufficient degree of frequency to justify prediction and to deserve the name of a law. The law merely describes or states what has happened and what may be expected to happen without attempting to give any explanation of these occurrences.

The explanation of an event consists of a statement of its dependent relation upon some antecedent conditioning factor which is theoretically capable of being observed independently of the event in question. Any statement that the thought of an object suggests the thought of another object because of their similarity implies that similarity is an entity which can be observed independently of and prior to the thought sequence which is to be explained. Similarity is an attribute of the thought sequence, and the attempt to explain an event in terms of one of its attributes involves the fallacy of the conceptual reification of that attribute.

This separation of the so-called 'primary laws' into the two classes of *descriptive* and *explanatory* effectually disposes of the absurd problem of the 'reduction' of one law to another. The outcome of all such discussions is the mere observation that the sequential phenomena which are described by the terms 'Law of Similarity' and 'Law of Contrast' can both be explained in terms of the Law of Contiguity. But the laws of similarity and contrast still remain, for the term 'reduction' does not mean that these laws are thus eliminated from the picture.

The number of descriptive laws needs to be extended beyond the Aristotelian tradition. Experimental psychology in its early stages was wont to establish and justify itself by

attempting to show that age-old problems could be readily and finally settled by the experimental method. The laws of association were thus investigated. A great variety of laws was discovered—the nature and number of such laws varying with the experimenter.

These lists included such laws as subordination, superordination, part and whole, attribute and object, and subject and predicate, in addition to the conventional Aristotelian categories. For those who are interested, I may refer to the tabular data given in Warren's *History of the Association Psychology*, pp. 248–255.

It is obvious on a moment's reflection that it is only the descriptive type of law that can be discovered by such an experiment. The number of possible laws is considerable. There are as many possible laws as there are logical and grammatical relations, and it is possible for the same associative sequence to be denoted in several diverse ways. It is also obvious that these descriptive laws are applicable only to ideational sequences, and not to all sorts of associational connections, as in the case of simple habits. In fact, one may seriously question the possibility of devising a set of distinctive descriptive laws that can be applied to the whole range of associative sequences.

I hold no brief for these descriptive laws. If they are to be used at all, they should be given a separate classification, their number should be increased, and their limitations should be noted.

The explanatory laws are to be divided into three classes, on the basis of what they purport to explain.

The first class attempts to explain the origin or formation of these stimulus and response connections. It attempts to state the factual conditions which are essential to the establishment of these connections. The law of contiguity, or simultaneity, obviously belongs to this class. This law states that the contiguous perception of two objects is a necessary precondition for the perception, or thought, of one to arouse the thought of the other. In the realm of conditioning, the law states that the new stimulus to which the response is to be

conditioned must be presented contiguously or simultaneously with the stimulus that elicits the response, in order for the conditioning to take place.

It has been noted that a stimulus may become associated with and elicit any one of a large number of responses. This connection between a single stimulus and a number of responses has been termed an associative system. Let us denote the stimulus by S and the various responses by A, B, C, D, and E. It has been noted that S will elicit B much more frequently than C, and C more frequently than D. We have thus two questions to answer, or two facts to explain. Why will a given stimulus elicit certain responses more frequently than it will others, and why will the elicited response vary from time to time? In other words, we have to explain the relative strength of the various connections within an associative system, and why the weaker connections are sometimes excited, rather than the stronger ones.

It is these two questions that the so-called 'secondary laws' are designed to answer. Brown proposed nine such laws. Seven of these were primarily concerned with the relative strength of connections, and two were designed to explain their variability of functioning. The first group included relative frequency, relative recency, and relative liveliness or vividness, which were perpetuated in this country by James—with the omission of the term 'relative.' These laws state that the relative strength of a system of associates, or the relative likelihood of their arousal, is a function, all other factors being equal, of the relative liveliness of the original experiences, or of the relative frequency or recency of their occurrence. Of the second group, James adopted but one—the varying emotional condition of the individual—which he termed 'emotional congruity.' This law has received but scant notice among later psychologists. Neither Brown nor James divided these laws into two classes on the basis of their explanatory significance.

While these laws were developed in connection with ideational connections, yet they are just as applicable to the whole range of stimulus and response connections. Watson

merely applied the well-known secondary laws of frequency and recency to the simple habits of animals. The fact is that any complex organism like man gets a number of diverse responses associated with, or conditioned to, the same stimulating object; and we have to explain why this stimulating object is more likely to arouse certain of these responses than others, and why the response does vary from time to time. The fact is true, irrespective of the nature of the stimulating item, and regardless of whether the response is an overt movement, a glandular activity, an emotional reaction, or an ideational response. In studying the phenomenon of conditioning in the laboratory, the connections established are often purely laboratory artifacts and function in a relatively isolated manner. In actual life, however, the higher organisms at least build up various modes of behaving to the same stimulating object, *i.e.*, various responses become associated with or conditioned to the same object, and the response that the organism makes to this object will vary with circumstances.

There are thus three classes of explanatory laws, and the number of laws in each class needs to be extended beyond the conventional list in order to give an adequate account of the phenomenon in question.

The conventional list of frequency, recency, and vividness does not adequately explain the relative functional strength of a system of associations. Brown's additional list of four laws is likewise inadequate for the purpose. The law of primacy has also been suggested on the basis of experimentation, and this has been contrasted with recency. Primacy and recency thus refer to ordinal positions in a series of presentations. But the second position is favored over the middle position, just as the first position is favorable in respect to the second. If we are to use primacy and recency as particular laws, we need just as many more such laws as there are ordinal positions in the series of presentations. Primacy and recency are but particular instances of a more general principle, *viz.*, the relative effect of ordinal position, and these two laws should be discarded in favor of the more

inclusive law or principle of which they are specific instances.

Again, this use of recency as contrasted with primacy differs from the conventional usage of Brown and James. Recency, as they used the term, referred to the time interval between presentation and recall. An item that is recent in respect to order of presentation is not necessarily recent in respect to length of interval between presentation and recall. For example, in a memory experiment a series of ten paired associates are presented three times, and the subject is tested for recall after an interval of five seconds. The tenth item in presentation may be the eighth item in recall, while the ninth item in presentation may be the first in the recall test. The tenth item is the more recent in respect to order of presentation, but it is the less recent with respect to the time elapsing between its presentation and recall.

The above ambiguity is involved in Watson's use of the term recency. An animal performs three acts in order, *viz.*, A, B, and C. C is the successful act, and the animal is then fed and again tested on the following day. C is more recent than B in respect to order of performance, and in respect to the next day's test. Does Watson mean that C is selected and fixated because of its closer temporal proximity to food satisfaction, or to the next day's performance? With respect to subsequent performance, the time interval for the successful act is 24 hours, and 24 hours and one second for the preceding wrong response. Surely this slight difference in time interval is hardly sufficient to account for the fixation of one act and the elimination of the other. Yet this is the conventional meaning of the term 'recency.'

Experimental psychology has isolated a number of conditions that influence the functional strength of associative connections. It has been discovered that the likelihood of correct reproduction is a function of a great number of conditions, such as the amount of time elapsing since learning (recency), the number of repetitions involved in learning (frequency), the length of material, ordinal position in a series of presentations, the temporal distribution of the repetitions, the whole or part method of learning, the health

of the subject, his intellectual, volitional, emotional and motor attitudes, etc. All of these conditions are usually referred to and discussed under the heading of 'learning and recall.' Writers of texts are wont to discuss frequency and recency under the heading of the 'secondary laws of association,' and in connection with the topic of 'fixation and elimination.' The principles involved are again utilized in the chapters on 'learning and recall,' but without being specifically referred to as recency and frequency, and without any reference to the previous topics. Recency and frequency are thus treated as isolated phenomena. It is our belief that all of these conditions that purport to explain the strength of associative connections should be grouped together, and that it is wholly immaterial whether we call them laws of association, laws of conditioning, or laws of learning or habit formation. If we are to preserve the associational terminology, this particular group of laws must not be confined to the conventional list of frequency, primacy and recency, but must be expanded to include many of the factors that are discussed under the heading of the 'laws of learning.' I have indicated the nature of some of these additions, but any list will necessarily be expanded with the progress of experimentation.

A second group of laws is demanded to explain the phenomenon of variability of functioning. Brown's 'varying emotion of the hour' and 'temporary diversities of state' seem to belong to this class. These laws are not sufficiently comprehensive to give an adequate explanation of the phenomenon. I would suggest two general principles—'variable objective context' and 'variable subjective conditions.' The first law states that the mode of responding to a given stimulating object tends to vary with all variations of the objective environment in which it is encountered. For example, the mode of responding to an acquaintance will tend to vary according to the environmental circumstances in which we encounter him—whether at his home, his office, at a club, at church, at a social function, or while travelling in Europe. The influence of the environmental context can be experi-

mentally verified. The second law states that the mode of responding to a stimulating object tends to vary with all sorts of fluctuating organic conditions, such as our rhythmical needs and appetites, our fluctuating interests, purposes, intentions, and moods, and presumably with many other conditions too numerous to specify. From the standpoint of behavior, the organism is continually changing, and its behavior is continually being affected by these temporal fluctuations of organic condition.

There still remains the task of explaining the origin of these stimulus and response connections. The task is that of discovering and stating the various factual conditions that must be fulfilled in order to establish these connections.

The usual explanation has been that of the law of contiguity. This law has been subjected to a considerable amount of critical discussion. It is now generally recognized that the law was originally stated in unfortunate terms. It was stated in terms of the spatial and temporal contiguity or proximity of two external objects rather than in terms of the temporal proximity of two mental events, two organic activities, or two items of experience. The statement also implied a relatively passive organism.

At present, every one recognizes that it is two organic processes or events that get connected in a stimulus and response relation. Two conditions, at least, are necessary for the establishment of such a sequence. They must occur in the same organism—a fact that is always taken for granted. Also they must occur with some degree of temporal proximity to each other. If the term 'contiguity' is to be used, it must be shorn of its spatial implications. It must denote temporal proximity, or 'togetherness in time.' It is for this reason that some writers prefer the term 'simultaneity.'

These critical discussions have apparently induced a few writers to discard the concept of contiguity entirely. Such a position is indefensible, for some degree of temporal proximity seems to be essential to the formation of a desired connection. When an experimenter attempts to condition a selected response to a given stimulus, he arranges his experimental

conditions in conformity to this principle. I have yet to hear of any experimenter who aroused the given response on one day and presented the novel stimulus a week later. In conditioning language responses to a conventional system of visual symbols as in learning to read, a successful teacher attempts to bring the two events—looking at the symbol and the vocal response—into close temporal proximity. I suspect that any teacher who systematically presented the visual symbol on one day and aroused the corresponding vocal response on the following day would soon find herself without a job. Any statement of the requisite conditions must include that of temporal proximity if it is to be adequate and complete.

What degree of temporal proximity is essential? According to the usual statement of the law of contiguity, the two events must occur simultaneously or in immediate succession, in order to be integrated into a stimulus and response sequence. The term 'immediate succession' is not defined, but it seems to imply the notion that the two events cannot be separated by any time interval.

Do the events need to be in temporal contact? There is a considerable amount of experimental literature bearing upon this question, and I shall briefly indicate the results of some of the more important experiments. In a memory experiment, Froeberg varied the time interval between the items to be associated from immediate succession up to five seconds. The items were associated, and the rate of formation increased with the time interval. It was noted that the subjects employed the time interval in a memory review, *i.e.*, the items were brought into temporal proximity by means of memory. He then introduced distractions during the interval, and found that the items were associated for all time intervals, but that the rate of formation decreased with the size of the interval.

In his famous cat experiment Thorndike found that he could establish an association between a sensory stimulus and a motor response over an interval of ten seconds. In a more systematic experiment with rats, Yarbrough established a

connection over an interval of six seconds which was the largest interval he used. Like Froeberg, he found that the rate at which this association was established decreased with the length of the interval. He also found that the association would be established when the stimulus item was presented either before or after the arousal of the response. The reader may be referred to Yarbrough's article [1] for a complete summary of the literature up to that date.

In the meantime Pavlov and his students had been working upon this problem independently. They systematically varied the time relations of the stimulus and the response in conditioning a reflex to a given stimulus. Their results are much like those obtained by Yarbrough.

It is admitted that the results obtained with human subjects are ambiguous in import because of ideational complications. There is the possibility of memory review, and there is always present the intention to memorize. Presumably these complications are not present in the animal experiments.

The results on animals, in my opinion, prove that two organic processes may become associated when they occur with a considerable time interval between them. What is the maximum limit of time that is requisite to the formation of an association? Is it possible under proper conditions to establish a desired connection over practically any time interval, *i.e.*, is there no time limit? There is no answer to these questions at present, but two comments are in order. (1) If there is no time limit, temporal proximity must be regarded merely as a favorable—and not as an essential—condition to the formation of these sequences. (2) If some limit of time is an essential prerequisite to the formation of these sequences, any statement of the law of temporal contiguity must be couched in terms of this limit, for any such law merely attempts to state the temporal conditions that are essential to the establishment of these connections. Obviously the usual formulation of the law of contiguity must

[1] Yarbrough, The influence of the time interval upon the rate of learning in the white rat, *Psychol. Rev. Monog. Sup.*, 1921, **30** (No. 2).

be considerably modified in the light of these experimental data.

Is temporal proximity a *sufficient* condition for the establishment of a stimulus and response connection? The implication of a passive organism in the original statement of the law of contiguity has been continually subjected to criticism. The critics have argued that any statement of the conditions essential to the formation of an association must include some sort of activity on the part of the organism in addition to that of temporal proximity.

Obviously, the organism is never passive in the sense of being inert. A living organism is necessarily active in one sense of the term, and activity of this sort is a necessary condition. It is equally clear that two simultaneously presented objects would never become associated unless the organism at least sensorially reacted to them. Activity of this sort, however, is taken into account when the law is stated in terms of the temporal proximity of two organic processes rather than in terms of two external objects.

It has been noted that the character of the associations that are established is not wholly determined by the sequence of objective events, but that it is also materially influenced by the reaction of the organism to its environment. An organism is active in the sense that it moves about and alters its spatial relation to the objective world. This behavior of an organism thus determines in part the character of its sensory contacts and their sequential arrangement. Behavior of this sort, however, is not a necessary condition to the formation of association in addition to that of temporal proximity. These reactions merely influence the nature of the connections that are established by determining in part what items are experienced in temporal proximity to each other.

It has been suggested that two sequential items will not become associated unless the organism reacts to them as a unit. This doctrine has been supported by a number of experiments. Woodworth presented his subjects with a series of items. The subjects were instructed to memorize these

as a series of paired associates, so that they would be able to recall the second member of each pair upon the presentation of the first. In half of the cases the subjects were presented with the first member of each pair and were requested to give the second member in accordance with the instructions given during learning. In half of the cases the subjects were presented with the second member of each pair and were requested to recall the first member of the succeeding pair. In the second case the items were presented in immediate succession and the subjects merely observed them, while in the first case the items were presented in immediate succession and the subjects presumably reacted to them as an associative unit. As might be expected, the presence of this reactive attitude materially increased the amount recalled.

This experiment does not prove, however, that such a reactive attitude is *essential* to the formation of an association, and that mere sequence is insufficient, for associations were established when only the sequential factor was present. The experiment proves that this type of reaction is merely a favorable—but not a necessary—condition for the formation of these associations.

On the other hand, the sufficiency of temporal proximity is proved by experiments on animals and children. In Pavlov's experiment the dog reacts to the food stimulus, while the sound of the bell is a mere incident to this behavior situation; and yet this incidental stimulus gets associated with the salivary response. Pain has been used to elicit a reflex defensive reaction, and an incidental sound has become associated with that response. In Yarbrough's experiment, the rat was taught to respond in a definite manner to an electric shock in order to obtain food. After this habit had been thoroughly established, a buzzer was sounded at the time the response was made. From the standpoint of the animal, this sound was a mere chance and *incidental* stimulus, having no significant relationship to the behavior situation; yet this sound became associated with the response. Blatz aroused a respiratory and circulatory response in human subjects by the sudden withdrawal of support. A buzzer was

sounded at the time of the reaction, and subsequent tests proved that this stimulus had become associated with the response. Watson's experiments in conditioning a fear response to a visual stimulus may also be cited.

These experiments prove that *incidental stimuli* which occur while the organism is responding in a prescribed manner may become associated with the response. They do not prove, however, that any incidental stimulus will become integrated into the ongoing activity. Perhaps the result will depend to some extent upon such factors as the nature and intensity of the stimulus, the abruptness with which it is given, and the unexpectedness of its occurrence.

These conditioning experiments do suggest that motor activity on the part of the organism is, after all, an essential condition to the formation of associations. The usual formulation states that two contiguous impressions become so connected that thereafter the presence of one arouses the thought of the other; or that two objects simultaneously perceived, get so connected that thereafter the perception or thought of one arouses the thought of the other. Sense impressions, or perceptual activities, are thus supposed to become directly associated with each other.

In the conditioning experiment, however, the two stimuli, or sense impressions, do not become directly associated with each other. One stimulus is already connected with the motor response, and it is presented merely as a means of arousing that act. The new stimulus is then presented, and becomes associated with the response. Both sensory processes are thus *directly* associated with the motor response, but they are not associated with each other. They are merely *indirectly* connected with each other by virtue of their direct association with a common response.

We may conclude that, so far as the early stages of mental development are concerned, sense impressions are invariably associated with motor or glandular activities, and that they are never directly associated with each other. We had previously noted that it is organic processes, and not objects, which are associated. The law of contiguity must now be

further restricted, for it is only sensory and motor processes that can be organized in a stimulus and response relation.

In the course of development any single sensory impression will become associated with a number of motor acts; and each act will become associated with a series of sensory stimuli. We may now suggest the hypothesis that this highly complex system of sensori-motor interconnections constitutes the basis for the ideational connections of later development. Percepts and ideas of objects arouse ideas of other objects, not because these objects have been directly associated with each other in sensory experience, but because the sensory impressions of these objects have been directly associated with a common motor response. Ideational connections find their origin in these indirect connections between sense impressions which accrue from their direct association with a common response.

A motor response on the part of an organism is thus an essential condition for the formation of associations. Sensory impressions can become directly associated only with a motor response, and a common response constitutes the basis for the indirect connections among ideational objects.

We may now raise the question as to whether the temporal proximity of a sensory and a motor process is the only condition under which they may begin to function in a stimulus and response relationship. Can all stimulus and response relationships be explained on this basis? I venture to suggest that other conditions are equally effective.

In the usual experimental procedure, the conditioning stimulus, C, is presented in close temporal proximity to the exciting stimulus, E, which is employed merely as a means of arousing the act that is to be associated with C. But if this conditioning stimulus, C, is highly similar to E, temporal proximity is no longer necessary, and C will tend to elicit the response at any time it is presented. Similarity of the two stimuli is now the effective condition of the stimulus and response relation. I have termed this the Law of Assimilation, and it may be formally stated as follows: Any novel sense impression will tend to elicit those responses that are

already connected with a similar sensory stimulus. For a detailed exposition and experimental proof of this law, the reader is referred to a recent article by Yum.[2]

Another effective condition is illustrated by Wylie's experiment on transfer. The rats were allowed to choose between two long paths, and the experimental conditions were arranged so that the animals were forced to continue in the chosen path when no signal was given, but to turn around and take the other path when the specified signal was given. After these habits had become thoroughly established, a new signal was substituted for the familiar one. An electric shock, the flash of a light, and the sound of a buzzer were employed as stimuli, and each of these was substituted for one of the others in all possible combinations. Under these conditions, the substitute stimulus tended to elicit the act of returning. The same principle is illustrated by the old reaction time experiment. When the subject is thoroughly prepared to react to an expected stimulus, the sudden inruption of any kind of stimulus will tend to release the act. Under certain conditions then, a sensory process may function in a stimulus-response relation with a given act although it has never been previously experienced in conjunction with that act, and although it bears no obvious similarity to any stimulus with which the act is associated.

We may now restate the general thesis of this paper. The formation and functional efficiency of stimulus and response connections at all levels of mental development constitute a legitimate and important psychological problem. It makes no difference whether we refer to this topic as a phenomenon of association or of conditioning. It is wholly immaterial whether we speak of the laws of association or of the laws of conditioning, or even of the laws of learning. In fact, we can talk of the conditions under which such connections are established, and of the conditions that determine their functional efficiency, without calling them 'laws.' Such terminological questions are relatively unimportant matters.

[2] Yum, K. S., An experimental test of the law of assimilation, *J. Exper. Psychol.*, 1931, 14, 68–82.

The technique that has been developed by the associational psychology for dealing with this general problem is inadequate for the purpose. The so-called 'laws' need to be re-classified on the basis of their function. The number of laws in each class needs to be considerably amplified. I have indicated some of the additions that are necessary, but I make no pretense of giving a complete and final list. Each list will necessarily be extended with further experimental work. Further progress is impossible with the customary method of reflective analysis of ideational connections, for we are here dealing with highly complex conditions involving a multitude of indirect connections. The problem must first be experimentally attacked with simple behavior conditions; and the more complicated conditions must then be studied from the standpoint of their genetic development.

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IS THE REFRACTORY PHASE THEORY ADEQUATE TO EXPLAIN MENTAL FATIGUE?

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Little interest has been shown of late in the theoretical aspect of the problem of mental fatigue. Many important studies have been made in the field of mental work, and the factors affecting the work decrement have been analyzed, but the word fatigue is for the most part avoided. Muscio (**18**), as well as Watson (**32**), recommends that the term be 'banished from precise scientific discussion.'

Many writers warn us against the identification of mental fatigue and the work decrement, and they restrict their discussion to the latter. Edward S. Robinson has a thorough paper on the *Principles of the Work Decrement* (**23**). As his title implies, he seeks not to explain fatigue but to describe objective phenomena. His fundamental assumption is the cumulative refractory phase. As Arthur G. Bills points out in his summarizing article in 1929 (**4**), 'Discussions of the results of continuous mental work on its efficiency tend, more and more, to relate the phenomena to the biological process of refractory phase, or the inhibitive effect of repeated stimulation of a simple neural process on its subsequent operation.' The theory seems to be taken for granted, and experimenters interpret their findings with it as a basis. Some of them comment, to be sure, upon the fact that mental work has a marked effect upon feelings of fatigue and only a slight effect upon the work decrement and the metabolic processes, but nowhere do they suggest that this discrepancy may offer grounds for suspicion of the theory.

Raymond Dodge and Edward L. Thorndike, more than the others, have been interested in the fatigue aspect of the problem. Professor Dodge has written of it with particular penetration. He analyzes refractory phase as an explanatory

principle, pointing out that it operates to prevent real fatigue. Hence mental fatigue is relative and not absolute, and its relativity is conditioned by two factors, the inconstancy of the stimuli and the interaction of the competing paths. These ideas he has embodied in his *Two Laws of Relative Fatigue* (7).

In his earlier work on the subject, Professor Thorndike seemed not to favor a theory so narrowly physiological. In 1923 (30), he emphasized the importance of 'satisfyingness' in his 'Biological or Extrinsic Theory.' 'Work without rest,' it maintains, 'becomes less satisfying (1) by losing the zest of novelty, (2) by producing ennui, a certain intellectual nausea, sensory pains and even headache, and (3) by imposing certain deprivations—for instance, from physical exercise, social intercourse, or sleep.' In a recent article, however, *The Refractory Period in Free Association* (31), he agrees explicitly with Professor Dodge's comments upon his experiment, and Dodge has declared himself whole-heartedly in favor of the refractory phase theory.

So there seems to be not one dissenting voice. As one paper puts it (24), 'That this same refractoriness is present throughout the range of psychologically described phenomena is too familiar a fact to need discussion.'

Yet the recent investigations made by Lashley and Franz upon the rat, with their rather startling implications for human beings, perhaps justify or even make necessary a reëxamination of this popular hypothesis as the sole explanatory principle of mental fatigue phenomena.

So far as the writer knows, no formal defense of the cumulative refractory phase theory has been published, presumably because its adequacy has not been challenged. Its strength seems to consist first in its fitting in with what Dodge calls 'the physiological tradition' (7), and secondly in its explanation of three types of psychological phenomena, (*a*) normal adult avoidance of repetition, (*b*) qualitative changes in the stimuli in long continued work, and (*c*) rapid onset of fatigue and a work decrement in homogeneous work.

Until recently the theory seemed unquestionably sound and satisfactory physiologically. With a nerve-muscle preparation, the refractory phase of the nerve can be clearly demonstrated. Sherrington defines it as 'a state during which, apart from fatigue, the mechanism shows less than its full excitability.' What could be more logical than to assume that in the more complex neural situation represented by a mental process, we have the same neural phenomena on a larger scale? The stimulus-response arc, upon repeated stimulation, presumably exhibits cumulative refractory phase, and mental fatigue with inability to continue work results until such time as permits recovery. This time may range from a second or two to a much longer period.

The theory is clear and consistent and has the undoubted advantage of simplicity. Unfortunately, recent findings seem to show that the actual situation is not so simple.

If Professor Lashley has published anything specifically on the subject of mental fatigue, the writer is unaware of it, but surely the pertinence of his experimental findings is shown by these excerpts (**15**):

> The maze habit, formed before the cerebral insult, is disturbed by lesions in any part of the cortex. The amount of reduction in efficiency of performance is proportional to the extent of injury and is independent of locus.
>
> It is very doubtful if the same neurons or synapses are involved even in two similar responses to the same stimulus. Our data seem to prove that the structural elements must be some sort of dynamic patterns, determined by the relations or ratios among the parts of the systems and not by the specific neurons activated.

If he is right, and if his conclusions regarding the brains of rats may be considered in some degree applicable to human beings, we can hardly retain the idea of rigid stimulus-response bonds promptly subject to refractory phase upon repeated stimulation. We must, on the contrary, conceive of refractory phase when it occurs as affecting the neurons of a whole pattern which has been aroused in its entirety. In that case it could hardly be either so rapid or complete as if only isolated stimulus-response systems were operating.

C. Judson Herrick (12) seems to feel that we are entirely justified in considering Lashley's discoveries regarding the brains of rats important for an understanding of human brains, and he emphasizes the even greater fluidity of performance in the latter.

> That which is most characteristic of human cortical activity is just that flexibility or plasticity of organization which facilitates the formation of innumerable, transient associational patterns which have no enduring quality.

In terms of mental life, revery is probably the example par excellence of these innumerable, transient associational patterns. Those times when we relax, when we are less subject than usual to outer stimuli, are precisely the times for the greatest variety of mental activity. The shifts are quick, and light, and continually changing.[1] It is as if ceaseless activity were the rule of mental life. It would seem that some principle must be operating here other than the relatively slow refractory phase.

It may be, then, that indications point to a theory of a more complex mental organization than is usually held. If refractory phase operates to forestall and prevent real fatigue, may it not be that frequently a more swiftly moving mechanism may forestall and prevent refractory phase? Much more knowledge is needed before such a theory can be worked out satisfactorily. Tentatively, however, some guesses are possible.

The whole cortex, let us suppose, tends to be intensely active, unstable or in a very delicate state of balance, and through it and over it dynamic patterns of greater or less complexity are continually playing and changing. When one pattern has been activated for a time, and the energy is touched off, as it were, in a good many of the neurons concerned in it, a lack of balance occurs, and there is a tendency to shift to other patterns so that equilibrium may once more be established. Since there is a possibility that each neuron

[1] Spearman (26) gives as his 'First Quantitative Law of Mental Energy': 'Every mind tends to keep its total simultaneous cognitive output constant in quantity, however varying in quality.'

in the pattern may be stimulated only once, there is little opportunity under normal conditions, when fatigue is not extreme, for refractory phase to occur. The slightest disturbance of balance is enough to produce a tendency to shift so that patterns can play over relatively large cortical areas. What this tendency to shift may be in its essence, our present knowledge does not permit us even to guess; but this very tentative hypothesis would suggest that the answer is to be sought, not only in the limited pattern that has been activated, but also in the condition (electro-chemical or whatever it may be), in the disturbed balance of the rest of the cortex. The tendency to activity appears, to be sure, in the form of a response to a stimulus, but according to this view the particular stimulus may be a relatively incidental matter. In the terms of the old figure, a good many thresholds are probably lowered. Facilitation will occur, then, when the patterns can be so integrated that they all lead, directly or indirectly, to the final common path. Inhibition will occur when the patterns are relatively isolated, so that instead of integration a sort of alternation will have to take place.

If we grant the physiological possibility of this hypothesis, we must recognize mental fatigue as being of a different nature from physical. We are prone to assume that both must be of the same nature because we generally experience them together and because we are unable to distinguish introspectively between the symptoms. But the slight effect upon metabolic processes of mental fatigue as compared with physical would certainly suggest that the two are different.

Feelings of mental fatigue, according to the present theory, are the real reflection of a physiological condition, but they occur much as feelings of restlessness do in our muscles, not so much because certain sections of our brains are overworked as because others have not been given a chance to work. In Thorndike's appealing phrase, we are like the child who was 'tired of not playing.' We call these feelings by various names, boredom, ennui, fatigue, and so forth. Frequently they are accompanied by sleepiness,

eye strain, and physical fatigue resulting from the general muscular tension that Dr. Bills (**2**) found present in mental work. At first sight this looks much like Professor Thorndike's biological or extrinsic theory, and it does make use of his data, but it is not extrinsic. It assumes that reactions take place not primarily because insistent stimuli present themselves, but because the nature of the cortex itself makes it intensely alive to whatever stimuli there may be. The emphasis is upon the physiological factor, not upon extraneous circumstance. If there is any truth in the hypothesis, it is no longer surprising that mental fatigue should be accompanied by so few measurable effects upon pulse rate, temperature, and other physiological functions.

Whether some modification of this theory or an entirely different one should prove ultimately to be satisfactory, this much seems evident: Professor Lashley's discoveries have cast grave doubt upon the tenability of the theory of rigid stimulus-response bonds which would quickly and constantly suffer refractory phase.

Does the refractory theory nevertheless offer so good an explanation of psychological phenomena that we should hold to it anyhow? As was stated above, the hypothesis has been employed chiefly to account for three types of phenomena: (*a*) avoidance of repetition on the part of normal adults, (*b*) the qualitative changing of the stimuli in a long continued task, and (*c*) the rapid onset of fatigue in homogeneous work. Dodge has perhaps given most attention to the first two of these problems, Robinson and Bills to the third.

In his article in 1927 (**9**), Professor Dodge makes the statement:

> If the principle of refractory phase does not apply to mental life, there is something so similar to it that it is not distinguishable from it. If physiologists had not discovered the principle, psychologists would have had to postulate it to account for the control of the perseverative tendency.

He calls attention to the fact that in children and savages there seems to be a tendency toward seeking repetition, which is not present in civilized adults. He explains it thus:

In some primitive or relatively less developed systems the refractory phase is apparently shorter than it is in developed and more complex systems. There is some evidence of this in the reactions of children or infrahuman species. Refractory phase may also be very short in certain dementias, which are characterized by stereotypy. High intellectual development seems to increase the barrier against repetition.

Why should higher intellectual development cause a change in the structure of the neurons so that the refractory phase would be lengthened? Isn't it simpler to look at the situation in another way? The sick or weak person is content with brief and limited movement; the vigorous one does not want his movement restricted. Is there anything necessarily mysterious in the fact that the child or feeble-minded person enjoys repetition, which is adapted to his capacities, and that the mentally vigorous adult resents it? It is difficult to see how one could account for the obsessions and repetitions of the insane on the basis of the refractory phase. Why should a pathological condition make nervous tissue less refractory? It is not difficult, however, to conceive a condition in which the whole mechanism should be rendered less flexible than usual, so that practically any stimulus would seem to lead to the same dominant pattern. We see the phenomenon in ourselves on a smaller scale at a time of great grief or despair, when almost everything serves as a reminder of our dominant idea. It hardly seems reasonable to assume that our cerebral tissue becomes temporarily less refractory.

Professor Dodge discusses the second point, motivation or qualitative changes in the stimuli, in his 'First Law of Relative Fatigue.'

Within physiological limits, all fatigue decrement in the results of work is relative to the stimulus.

In mental work we are often distinctly aware of changes in the intensity of the inner stimuli that keep us at a disagreeable or monotonous task. Mere interest in the task may lose its force comparatively early. Then the task is continued from stubbornness, the dislike to fail, sense of obligation, honor, fear of ridicule, or the hope of reward, etc.

All of us recognize the truth of this description, but Dodge does not explain whence come the fresh stimulations. To say that a given inner stimulus loses its effectiveness because it suffers refractory phase does not explain why another inner stimulus should immediately become operative, nor why either should be able to postpone refractoriness in the original neural arc. There is at least the suggestion of an explanation in this passage from Lashley:

> The relation between efficiency of performance and mass of tissue implies some sort of general facilitation. There is some indication that activation of a habit involves also a partial activation or increase in central tonus of all closely related habit systems, with the possibility of mutual facilitation and the determination of the general level of activity by the total mass of excitation.

There would then, in long continued effort, be the possibility of refractory phase in all the neurons of a system, which in its entirety might have been in a state of partial excitation. There would also be the possibility in such a case of ready reinforcement of the system activated originally on the part of related habit systems.

This point certainly seems to be borne out by Dr. Bills' experiment of 1927, *The Influence of Muscular Tension on the Efficiency of Mental Work* (2). He found that muscular tension not only usually accompanied mental efficiency but actually increased it. Since two or more habit systems can function together with mutual facilitation and generally draw upon other related systems as well, it seems no great step to conceive of motivating forces as being habit systems which can be drawn into the general integration about a particular pattern.

Such a conception offers a rather intelligible explanation of the marked differences in fatigue we experience upon various occasions. We may be worn out after two hours of listening to boring conversation, yet feel fresh and highly stimulated after a similar period of strenuous mental effort that has brought results to our liking. In the first case, the topics discussed may be so trivial as to arouse very limited patterns in us, or so big that they cannot stimulate

many patterns in us because of our limited experience. In the second case, the work may arouse in us patterns involving a good deal of our past experience, together with the self-feeling patterns of pride in success, desire for approbation, and so forth.

The whole problem of motivation is tremendously complex and its ultimate solution probably very far off, but in any given situation, is it not at least possible that the force of a motivating influence is relative to the size of the pattern involved in it? It would follow that the more numerous and strong the motives involved, the larger would be the resulting integration, and the less the fatigue experienced.

If the refractory phase theory, then, is unnecessary in explaining adult avoidance of repetition and is not sufficiently flexible to explain changing motivation, what can be said of it as regards the third point, the rapid onset of fatigue in homogeneous work?

That such work does rapidly produce feelings of fatigue and a work decrement, there is no possible doubt. Professor Dodge has based his 'Second Law of Relative Fatigue' upon the fact, and it has been found repeatedly in the many recent studies of continuous, repetitive work, as Dr. Bills shows in his 1929 article (**4**). Such results can be explained very conveniently by means of the refractory phase. One can readily understand indeed, with Professor Dodge, that psychologists might want to invent the theory to fit these facts if it did not already exist. There are certain phenomena, however, less important than these, perhaps, but indubitably present in homogeneous work, that the cumulative refractory phase theory seems not so well adapted to explain.

The first of these has already been mentioned, that is the discrepancy between the slight amount of the usual work decrement and the marked feeling of fatigue. This has long been noted and commented upon. Thorndike says (**28**):

> The most important fact about the curve of efficiency . . . is that it is so near a straight line and so near a horizontal line. The work grows less satisfying or more undesirable, but not much less effective.

This fact is reasonable enough on the basis of his extrinsic theory; but as Professor Robinson points out (**23**), 'Professor Thorndike's suppositions are not very exhaustive or very specific.' If the refractory theory holds, why are the objective effects relatively so slight? A satisfactory answer has at least not yet been suggested. If, on the other hand, a tendency to general cortical activity would ordinarily prevent the occurrence of refractory phase, one would expect only a slight decrement—about as much as would normally result from the physical fatigue incident to mental work, due to muscle tension and so forth.

The other phenomena that seem difficult to account for by means of the refractory phase theory are found in recent careful experiments. In *Two Factors in the Work Decrement* (**24**), Robinson and Bills call attention to their subjects' reports of their introspections.

> Eleven of the eighteen subjects noticed that the letter naming went on much more smoothly while they were engaged in apparently irrelevant trains of thought. These ideational activities seem to have been of the order of fairly concrete fantasies.

Apparently a certain degree of automatization takes place which renders repetitive work less fatiguing. This conclusion is confirmed by the results of an unpublished study of Dr. Bills, in which the subjects worked at substituting or color naming. Blocks occurred frequently with both types of work. This fact would seem to confirm the refractory phase theory, but the subjects' introspections reveal that in these periods of blocking there is not blankness except in so far as the desired response is concerned. There occur various self-feelings, fatigue, exasperation, amusement, interest in what the experimenters are doing, consciousness of temperature, and recognition of preceding and following stimuli in the list. All of these are fleeting, but they occur, and then the subject can give the desired response, and can continue through a whole group of responses at much his former rate. With greater practice the mental content during blockings becomes less sharply differentiated from that during responses. A sort of drowsy revery seems to pervade both. When the mo-

notonous tasks were continued for an hour, the results were rather astonishing. The subjects experienced not more but less fatigue at the end of the period than after the first ten or fifteen minutes.

It is difficult to see how these facts of partial automatization can be accounted for by the refractory phase. If we could be sure that habitual acts are relegated to lower than cortical levels, it would be easier; but Professor Lashley's experiments (15), showing that a portion of the cortex is necessary for the retention of the rat's maze habits, discourages that belief. To be sure, it is not possible to draw absolute conclusions regarding human beings from data regarding rats, but since in most respects the cortex seems to be increasingly important in the higher forms of life, there is at least no a priori reason for concluding that habitual acts are transferred to lower levels in the human being. It would seem particularly far-fetched to assume it in the present instances where the activities involved are of a visual-vocal nature.

Might not the general activity hypothesis account for these phenomena? The blocking gives opportunity for wide patterns of excitation, of short duration, to be sure, but of some variety, after which it is possible again for the subject to give the desired response in the restricted pattern. There is time at first for only extremely fleeting play of these other patterns, but as automatization is approached, and the stimuli with their appropriate responses become partly integrated with some of the dominant patterns of self-feeling, these reliefs become more and more possible and continuous, and hence fatigue effects are less and less painfully noticeable.

The hypothesis put forth in this paper is extremely sketchy and tentative in character, but it seems more in harmony with recent neurological findings than the theory of refractory phase, and it seems also to account rather better for certain psychological phenomena. Its main points may be summarized as follows:

1. There is a general tendency toward wide-spread cortical activity, which ordinarily prevents the occurrence of refractory phase.

2. If the cortical pattern activated at a given moment is relatively unrestricted, there is a tendency to integrate with it various other patterns, so that the total integration may have a considerable duration.

3. If the cortical pattern activated at a given moment is relatively limited in extent, there is a tendency toward rapid shifting to other patterns, accompanied by feelings of fatigue, until such time as practice is able to effect a partial integration with dominant habit patterns.

Whether or not this hypothesis may point the way toward a satisfactory theory of mental fatigue, the cumulative refractory theory as the sole explanatory principle, at least, does not seem adequate. It falls short in two main respects:

1. Physiologically its rapid and constant operation implies a rigidity of cortical organization that is difficult to reconcile with Professor Lashley's findings.

2. Psychologically it seems over-elaborate to account for adult avoidance of repetition, is not well adapted to explain the plastic and powerful nature of changing motivation, and is at variance with the complex phenomena of partial automatization.

Bibliography

1. Arai, T. Mental fatigue, Teachers College, Columbia University, Contributions to Education, 1912, No. 54.
2. Bills, A. G. The influence of muscular tension on the efficiency of mental work, *Amer. J. Psychol.*, 1927, **38**, 227–251.
3. Bills, A. G. Inhibition and facilitation, Psychol. Bull., 1927, **24**, 473–487.
4. Bills, A. G. Mental work, Psychol. Bull., 1929, **26**, 499–526.
5. Bills, A. G. and Brown, C. The quantitative set, J. Exper. Psychol., 1929, **12**, 301–323.
6. Chapman, J. C. The effect of rapid changes of work on the rate of performance, J. Exper. Psychol., 1917, **2**, 165–170.
7. Dodge, R. The laws of relative fatigue, Psychol. Rev., 1917, **24**, 89–113.
8. Dodge, R. Mental work, Psychol. Rev., 1913, **20**, 1–12.
9. Dodge, R. Note on Professor Thorndike's experiment, Psychol. Rev., 1927, **34**, 237–240.
10. Franz, S. I. and Lashley, K. S. The retention of habits by the rat after destruction of the frontal portion of the cerebrum, *Psychobiology*, 1917–1918, **1**, 3–18.
11. Garth, T. R. Mental fatigue, *Arch. Psychol.*, 1918, **26**, 1–85.
12. Herrick, C. J. The brains of rats and men, Chicago, University of Chicago Press, 1926.
13. Jersild, A. T. Mental set and shift, *Arch. Psychol.*, 1927, No. 29, 1–82.

14. LASHLEY, K. S. Basic neural mechanisms in behavior, PSYCHOL. REV., 1930, 37, 1–24.
15. LASHLEY, K. S. Brain mechanisms and intelligence, Chicago, University of Chicago Press, 1929.
16. LASHLEY, K. S. The relation between cerebral mass, learning, and retention, *J. Comp. Neur.*, 1926, 41, 1–58.
17. LASHLEY, K. S. AND FRANZ, S. I. The effects of cerebral destruction upon habit formation and retention in the albino rat, *Psychobiology*, 1917–1918, 1, 71–139.
18. MUSCIO, B. Is a fatigue test possible, *Brit. J. Psychol.*, 1921, 12, 31–46.
19. MYERS, C. S. Conception of fatigue and adaptation, PSYCHOL. REV., 1925, 32, 370–380.
20. PAINTER, W. S. Efficiency in mental multiplication under extreme fatigue, *J. Educ. Psychol.*, 1916, 7, 25–30.
21. POFFENBERGER, A. T. The effects of continuous mental work, *Amer. J. Psychol.*, 1927, 39, 283–296.
22. ROBINSON, E. S. Mental work, PSYCHOL. BULL., 1921, 18, 456–482.
23. ROBINSON, E. S. Principles of the work decrement, PSYCHOL. REV., 1926, 33, 123–134.
24. ROBINSON, E. S. AND BILLS, A. G. Two factors in the work decrement, J. EXPER. PSYCHOL., 1926, 9, 415–444.
25. SHERRINGTON, C. S. The integrative action of the nervous system, New Haven, Yale University Press, 1906.
26. SPEARMAN, C. The nature of intelligence and the principles of cognition, London, Macmillan and Co., 1927.
27. SPENCER, L. T. The curve of continuous work, PSYCHOL. BULL., 1927, 24, 267–272.
28. THORNDIKE, E. L. The curve of work, PSYCHOL. REV., 1912, 19, 165–194.
29. THORNDIKE, E. L. The curve of work and the curve of satisfyingness, *J. Appl. Psychol.*, 1917, 1, 265–267.
30. THORNDIKE, E. L. Educational psychology, Vol. III, New York, Teachers College, Columbia, 1923.
31. THORNDIKE, E. L. The refractory period in associative processes, PSYCHOL. REV., 1927, 34, 234–236.
32. WATSON, J. B. Psychology from the standpoint of a behaviorist, Philadelphia, Lippincott, 1919, 348–392.

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THE CONCEPT AND CRITERIA OF INSIGHT

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Terminological difficulties are a notorious source of confused thinking. Psychologists are particularly sensitive to this fact since the technical vocabulary of the profession consists so largely of specialized meanings for ordinary words. This condition has been viewed with regret by the strict logicians among them and with indifference by most specialists, who incline to the opinion that the advance of rigorous experimentation will define the meanings more sharply or that the context of the study serves to give the denotation implied. While this latter view may satisfy the practical objectives of a limited study, it is fraught with danger where broader theoretical issues are involved.

The notion of insight as elaborated by the Gestalt psychologists during the last decade is a special case in point. Just as intelligence and instinct constituted the major theoretical controversies of the early 'twenties, so the question of the nature and function of insight appears to be one of the bigger problems of current research. It is the opinion of Boring (3, 464) that detailed investigations of insightful behavior have suffered from vagueness or ambiguity of conception and a lack of guiding principles. A cursory examination of the literature on insight shows that both precision and uniformity of meaning are absent. The primary task of this paper is to attempt, by means of a comparative and historical analysis, a reduction of the term to the level of a 'clear and simple idea.'

Examination of recent usage reveals at least three important meanings of insight. Since they all occur so frequently and possess an established respectability, it will be profitable to distinguish them sharply at the beginning of this discussion.

(1) Insight as general comprehension. Psychologists have long used the word in its popular or literary sense. One can find innumerable passages in both the older and newer writers to show that it is intended to be synonymous with rough understanding of a complex situation. Where the word is used without qualification it seems to be equivalent to an intuitive form of knowledge. A semi-technical employment of the term occurs in the writings of the *Geisteswissenschaft* school (e.g., Erismann) where it merges with the meaning ordinarily attached to empathy.

(2) Insight as a personality trait. Psychiatrists have long sought to discriminate between the functional disorders in psychotic and neurotic cases on the basis of awareness of one's condition. The sufferer from a neurosis is maladjusted and knows it; the frankly insane person is said to be relatively oblivious to his abnormality. The Allports have modified this view in their treatment of the social phases of character to designate the ability of a person to appraise his individual resources in accordance with the judgments of his fellows. Insight in this case has fundamentally an egocentric reference.

(3) Insight as configurational learning. This is now the commonest implication of the term and it is largely with its significance that we shall be concerned. One cannot be positive as to who first employed the word in connection with novel behavior, but that it is much older than Gestalt is evident from the following passage from Ruger's (11, 32) study of puzzle solution:

> Of course there is no mechanical way for the production of insights, but the conscious attempt to get into a different attitude, to realize that there may be other possibilities and to search for them, may be effective as a stimulus.

Woodworth, who served as Ruger's main subject, had already used the term in his lectures of 1908 in contrasting human and animal modes of learning.

Where a word is so old, it is quite certain that the fact it represents is even older. The term does not appear in Thorndike's (13, 16) classic account, but apparently he

observed something very much like insight which his theoretical background led him to interpret otherwise:

> The difference between these records [for monkeys] and those of the chicks, cats and dogs . . . is undeniable. Whereas the latter were practically unanimous . . . in showing a process of gradual learning by a gradual elimination of unsuccessful movements, and a gradual re-enforcement of the successful one, these are unanimous, save in the very hardest, in showing a process of sudden acquisition by a rapid, often apparently instantaneous, abandonment of the unsuccessful movements and a selection of the appropriate one which rivals in suddenness the selections made by human beings in similar performances. It is natural to infer that the monkeys who replace suddenly a lot of general pulling and clawing by a single definite pull at a hook or bar have an idea of the hook or bar and of the movement they make. The rate of their progress is so different that we cannot help imaging as the cause of it a totally different mental function, namely, free ideas instead of vague sense impressions and impulses.

Despite these earlier evidences of both the conception and verbal description of 'immediate' or 'direct' learning, the problem failed to receive further attention because the dominant theories of the day regarded it either as a minor freak of behavior or else as fully translatable into traditional 'bond' terms (*cf.* Thorndike's discussion of Ruger's experimental results in the second volume of his Educational Psychology). It was not until the publication of Köhler's observations at Teneriffe that the word became prominent in psychological colloquiums. Apparently it won such instant favor not only because it offered a unique and persuasive explanation of certain remarkable animal performances, but also because it embodied one of the main tenets of the new school of Gestalt.

Specifically, Köhler showed that under certain experimental conditions—particularly those in which all the essentials to a solution lie within the same optical field—an ape is capable of learning a task *with one trial.* To use Köhler's own words (**6**, 191): "Die Schimpansen zeigen einsichtiges Verhalten von der Art des beim Menschen

bekannten."[1] More specific indices such as permanence of retention, transfer to formally similar situations, etc., have been used to mark off this type of behavior from that which trial-and-error and mechanical linkage serve to characterize. Insightful behavior occurs when meaningful wholes (= sachlicher Zusammenhang) are established, while sheer associationism appears when there is no necessary connection between the event which occasions the solution and the solution itself. Hume's doctrine of causality is valid under the latter conditions, but altogether false in the case of the former.

Originally, then, the concept of insight was restricted to the activity of learning. Its criterion was the occurrence of complete solutions with reference to the total organization of the field. However, there has lately arisen a tendency to broaden the term to include many related phenomena in the field of perception and thought. In fact, the distinction between these divisions of mental life have all but disappeared in the Gestalt writings where insight seems to serve as a common factor to signify the formation of any new but adequate configuration.

It is rather curious to observe that the experimental testing of the insight concept has been largely in the hands of the American animal psychologists. Possibly this is to be explained as a reaction against the early excesses of behaviorism, but it is more likely that this receptivity resulted from a similarity of the independent findings of American and German psychologists. The 'insight' of Köhler's apes is paralleled chronologically (or even antedated) by the 'ideational behavior' of Yerkes' orang.[2] Both investigators have acknowledged that they were working along the same lines and that they reached practically identical conclusions.

[1] The German substantive 'Einsicht' and its adjectival form can be used with much less danger of clumsy repetition than its English equivalent. That there is no compunction about its frequent use is apparent from the fact that it appears seven times on one page of an untranslated article by Köhler (7, 88).

[2] For completeness' sake one ought to mention the earlier observations of Boutan on the gibbon. Unfortunately for his reputation, this report is most effectively buried in an obscure French journal. See Yerkes (16, 96).

However, the term insight appears to have had greater descriptive potency than the tradition-burdened phrase 'ideational behavior,' since Yerkes and his pupils have all but abandoned the latter designation in favor of the former. It is to this group of American workers that we owe particularly the development of the all-important criteria of insight which Köhler and his compatriots never outlined so explicitly.

Yerkes (16, 156) has drawn up the following list of the earmarks of insight:

> In acts which by us are performed with insight or understanding of relations of means to ends, we are familiar with certain characteristics which are important if not also differential. These features are: (1) Survey, inspection, or persistent examination of problematic situation. (2) Hesitation, pause, attitude of concentrated attention. (3) Trial of more or less adequate mode of response. (4) In case initial mode of response proves inadequate, trial of some other mode of response, the transition from one method to the other being sharp and often sudden. (5) Persistent or frequently recurrent attention to the object or goal and motivation thereby. (6) Appearance of critical point at which the organism suddenly, directly, and definitely performs required adaptive act. (7) Ready repetition of adaptive response after once performed. (8) Notable ability to discover and attend to the essential aspect or relation in the problematic situation and to neglect, relatively, variations in non-essentials.

Bingham (2, 43–45) has offered what is probably the most extensive and elaborate list of details which characterize insightful performances. It agrees in all essentials with that offered by Yerkes.

As a whole, I believe the following criteria are pertinent to a definition of insight:

1. Experience. A problem situation in which the crucial experience must be acquired with the solution probably provides most favorable controls for investigating evidences of insight.
2. Versatility.
   a. General inspection.
   b. Abrupt changes.
   c. Sudden initiation. In addition to abrupt changes in exploratory behavior, there are moments or periods when the behavior appears inspired. This is commonly indicated by marked

change in facial expression, particularly the lips, and sometimes by a squeal or sharp cry.

d. Correction of errors.

3. Consistent orientation.

4. Anticipatory responses.

5. Representative behavior.

a. Subordination of the final step until a preceding stage is reached.

b. Abandonment of readiest means that is blocked and pursuance of a round-about course that is open.

6. Sudden change in the time curve.

a. Drop occasioned and followed by methodical and consistent performance.

b. Rise occasioned by exploratory interruptions of consistent procedures.

7. Selection of goals.

8. Fluent solutions.

Apparently the insight factor is an exceedingly complex one if it offers such resistance to condensation within a single definition that it can only be communicated by a lengthy list of attributes. It cannot be defined like iron as one of the metallic elements, but instead must be described like the medieval God as omnipotent, ubiquitous, ineffable, etc.

A slightly different version is discernible in Tolman's account. Unlike Yerkes, he does not distinguish between insight and foresight, and as may be suspected from his purposive approach, tends to find a reference to ends as the common characteristic of trial-and-error learning and insightful learning. Differentiation is made on the basis that primary or insight solutions are new adjustments which occur *without* overt behavior whereas secondary or trial-and-error solutions come only *through* overt behavior (14, 48).

An odd feature in connection with the use of the insight concept is that without any thoroughgoing knowledge of the conditions of insightful behavior in man the term was almost compulsively applied to animal conduct wherever presumably human patterns of conduct occurred. No doubt the two fields of inquiry can be pursued relatively independently, but

it seems almost axiomatic that our understanding of animal psychology is limited more by the defects of our acquaintance with human psychology than vice versa. Hence the high value of those studies which clarify the facts on the human plane.

Alpert (1, 39) applied a variation of Köhler's stick and box-building devices to children aged 19–49 months with confirmatory results, but added the important modification of partial and gradual insight. One is tempted to believe that where insight is not immediate or at least sudden it has lost its essential character. If the word is made so flexible it becomes merely another designation for the noetic operations of the organism. One may also suspect that where partial and gradual insights are admitted they represent either what would ordinarily be termed sheer trial-and-error or at best transition stages between that and insight *per se*.

The work of Kreezer and Dallenbach (9, 436) is of interest in showing that the relation of opposites is learned by varying percentages of children between five and seven-and-one-half years after but one or two illustrative examples. A sort of all-or-none law seems to function on this level as the presence of insight was a *sine qua non* for mastering the task. Those who learned, learned by insight, and those to whom the insight did not come, did not learn at all. The only danger in such a bald statement of the fact is that it suggests that the solution itself becomes the criterion of insight—which conclusion, as a matter of fact, some persons have not hesitated to draw.

There is some reason for identifying insight with visual structure-functions since all the known experiments have been of that order. Köhler's papers on the optical properties of the hen and chimpanzee preceded his *Intelligenzprüfungen*, but the main conclusions are alike, namely, that responses to relations dominate over responses to elements. Helson (4, 384) has not hesitated to use the words interchangeably. This dubious proceeding becomes plausible, however, when we realize that he took as his criterion of insight the ability to transpose structures. Granting that the distinction be-

tween perception and thought is hard to draw, it appears desirable to restrict structure-function to simple sensory discrimination following training and insight to immediate learning without specific antecedent practice; both usages would have the advantage of adhering closely to the original situations out of which they arose. Needless to say, 'immediate' here is not equivalent to 'instantaneous'; instead, it refers to a sharp and unmistakable inflection in the learning curve.

That varieties of meaning should be attached to the same word by different writers is not surprising, but that inconsistency in its employment by one scholar should be so prominent is a symptom of the troublesomeness of the idea. For example, in Ogden (**10**, 126) the technical and colloquial uses of the term are badly mixed as any consulter of the index can verify. He offers this excellent general illustration of the word, but his ensuing discussion is only remotely pertinent to it:

> When I stumble over a stone in my path, I lack insight regarding the stone; but if I look at it, seize it, or walk round it, I possess insight, either at the elementary level of doing these things heedfully, or at the more advanced level of doing them by habit as a result of previous heedful behavior.

But bishops cannot be reproached for fluctuations in doctrine when the pontiff himself is not distinctly understood. In his *Intelligenzprüfungen*, Köhler used the word as a special descriptive term for the perception of significant relations—the fact, e.g., that a stick inevitably serves to close the gap between the ape's fingers and the objective. The intelligence which appears here, lies in the capacity for insight, or the ability to form the required configuration.[3] However, a broader scope is apparently desired for the concept if one reads aright the chapter devoted to this subject in Köhler's recent book in English. I quote at some length in order to show that the more generalized conception was intended even at the start, and that it was merely the accident of illustrative

[3] Wyatt has shown that intelligence does not lie in the configuration as such, but in the shaping-according-to-purpose (**15**, 378).

material which inadvertently restricted later discussion to the animal field:

> A great deal of purely sensory organization may be called 'silent' because though we experience the result of it as segregated wholes with specific properties, we do not usually feel *how* this result is dynamically brought about. In this respect the total field is different; it tells us more about its innermost nature. Here, not only the result is experienced, but also very much of its 'why' and 'how' is felt in just the actual context. Wherever this is the case we apply the term 'insight.' When I used this term in my treatment of intelligent behavior in apes, I ran the risk of an unfortunate misinterpretation. . . . Insight does not mean more than our experience of definite determination in a context, an event or a development of the total field; and in the actual cases there need be nothing like an invention, or a new intelligent achievement, or so forth. A total field would be experienced *without* insight, if all its several states, wholes, attitudes, etc., were simply given as a pattern, in which none was felt directly to depend upon any other and none to determine any other (8, 371–372).

Presumably this may be correctly re-stated as meaning that all experience is saturated with causal properties, and that these causal properties are themselves experienced Evidently the ample boundaries of human and animal learning have been transcended by this new emphasis. Yet this by no means implies an abandonment of the hard-headed factual standpoint for the allurements of fancy. As usual, Köhler tries to go a step further. The neurological foundations of the process of insight are explained in terms of the now familiar dynamic theory:

> When, on a hot day, I enjoy a cool drink, my enjoyment is felt to refer to, or to be based upon, the properties of the drink and my thirst, but not to the spider on the wall, nor to the size of a chair, nor to thousands of other things. In the brain-field, more particularly in that part of it which corresponds to the self, there is a special process B, felt as thirst in experience. Now when I begin to drink, another process A, corresponding to experienced coolness and taste, develops in the same region of the brain-field (experienced interior of mouth) where until now there was only the process of 'thirst.' In terms of the theory of insight and of direct

physiological determination, A begins at once to exert an influence upon B, the influence depending upon the concrete properties of A and those of the actually existing state B. The change, which is produced, is felt as pleasure (and, concomitantly, decreasing thirst). (8, 375–376.)

The different schools of psychology have each interpreted the insight experience in characteristic fashion, either by assimilating it to their own existing doctrines or by rejecting it as superfluous. Spearman is an interesting example of the first group with his positive assent to the main point and re-wording of the phraseology. Spearman obviously views it as confirming his second noegenetic law, the eduction of relations, which one must acknowledge comes as a highly plausible alternative (**12**). Ordinary behaviorism, however, as represented by Hunter (**5**, 575–581) takes a negative position toward insight and objects to it as an interpretation not warranted by the facts, or at least as an unnecessary supplement as long as the well-established principle of conditioned reflexes makes the more convincing explanation.

It is evident from such an assemblage of experimental and theoretical material that the subject of insight suffers from many serious defects. In the writer's opinion, the reason for this lies in the fact that the insight experience on the human level—the 'aha' phenomenon—is a terra incognita. The term was prematurely but inevitably applied to animal behavior before enough was known about its assumed human correlate. It is not enough to say that insight is opposed to trial-and-error in the same sense that logical memory is differentiated from rote memory. We need more than analogies to make this troublesome matter plain.

Among the many preliminary questions which must be settled before we can attain a satisfactory theory of insight we may list the following:

1. Are specific insights the outcomes of prior trial-and-error?

2. Does all learning require some insight, even of a rudimentary sort?

3. What is the best single criterion (or group of criteria) of insight?

4. Is insight necessarily accompanied by ideas? If so, what is their character?

5. Is insight an active production of the organism, or does it arise spontaneously?

6. Is the moment of insight emotionally-toned? What is the nature of this coloring?

7. Can insight be identified with association by similarity?

8. Is insight a species of the genus intelligence, or vice versa?

9. How is insight related to inference and induction?

10. What is the connection between closure and insight?

The available experimental data tell us very little about any of these points. It is quite clear that an extensive program of research will be necessary before even the simplest of these problems can be answered.

Summary

Current investigations and analyses of insight suffer from a lack of clear-cut conceptions and definite criteria for recognizing the experience. An examination of the difficulty shows that it is caused by an original terminological ambiguity and an historical shift of emphasis and meaning. A major source of confusion is that the word pre-supposed a unanimity of opinion about its features in the human subject at the time it was casually applied to describe animal behavior of a special sort. We are now in the curious position where objectively more is known about insight in animals than in man. To be sure, the implication is that the characteristics hold for both, but that is an assumption which must first be demonstrated. Our greatest present need is knowledge about the operation of insight in the human, the absence of which constitutes an effective check to progress in the animal field. The paper ends with a representative series of inquiries which must be undertaken before a suitable theory of insight can be proposed.

REFERENCES

1. ALPERT, A., The solving of problem-situations by pre-school children, New York, 1928.
2. BINGHAM, H. C., Selective transportation by chimpanzees, *Comp. Psychol. Monog.*, 1929, 5, No. 4.
3. BORING, E. G., History of experimental psychology, New York, 1929.
4. HELSON, H., Insight in the white rat, J. EXPER. PSYCHOL., 1927, 10, 378–396.
5. HUNTER, W. S., Experimental studies of learning, *Foundations of Experimental Psychology*, Worcester, 1929, pp. 564–627.
6. KÖHLER, W., Intelligenzprüfungen an Anthropoiden, Berlin, 1921. 2nd ed.
7. KÖHLER, W., Die Methoden der psychologischen Forschung an Affen. Handbuch der biologischen Arbeitsmethoden, Berlin, 1921.
8. KÖHLER, W., Gestalt psychology, New York, 1929.
9. KREEZER, G. AND DALLENBACH, K. M., Learning the relation of opposition, *Amer. J. Psychol.*, 1929, 41, 432–441.
10. OGDEN, R. M., Psychology and education, New York, 1926.
11. RUGER, H. A., Psychology of efficiency, New York, 1910.
12. SPEARMAN, C., The new psychology of 'shape,' *Brit. J. Psychol.*, 1925, 15, 211–225.
13. THORNDIKE, E. L., Mental life of monkeys, PSYCHOL. REV. MONOG., 1901, 3, No. 5.
14. TOLMAN, E. C., Habit formation and higher mental processes in animals, PSYCHOL. BULL., 1928, 25, 24–53.
15. WYATT, H. G., Intelligence in man and ape, PSYCHOL. REV., 1926, 33, 375–384.
16. YERKES, R. M., The Great Apes, New Haven, 1929.

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CINEMATOGRAPHY OF PSYCHOLOGIES

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Psychology or Psychologies?

'Psychology'? No, 'Psychologies'! As some critic has very significantly remarked, psychology first lost its soul, then its mind, then its consciousness, but nevertheless was careful in exhibiting sporadic behavior. What has become of this residual behavior? It is no longer sporadic. It has become more and more articulated, obsessed, and stereotyped, developing into systems and doctrines as clearly defined as their respective 'founders' could make them. In a word, it has assumed multiple personalities!

Introspectionism, functionalism, behaviorism, and purposivism; *Gestaltpsychologie* (*Strukturpsychologie*), *Entwicklungspsychologie*, *geisteswissenschaftliche Psychologie*, *verstehende Psychologie*, *persönliche Psychologie*, and *eidetik-typologische Psychologie*—This is impersonal designation. But Psychologies are far from impersonal. So we may as well learn the names of their sponsors: Wundt or Titchener, Angell, Watson, and McDougall; Wertheimer-Koffka-Köhler, Krueger, Spranger, Jasper, Stern, and Jaensch. There are still the varieties of Freud's, Jung's, and Adler's Psychoanalysis. Normal, abnormal, social, educational, industrial, legal, medical, child or genetic, animal or comparative are also Psychologies. Parapsychology; and pseudo-psychologies of 'magic and divination,' 'afternoon tea,' 'the market place,' and 'popular magazine and newspaper'; Psychologies of 'clinic,' 'the meter-stick,' 'textbooks,' 'the laboratory and monographs'—more caricature,[1] it is true, but how real!

The word 'psychology' has by no means exhausted its usefulness. We have also technically Psychologies of feeling,

[1] M. Bentley's, Introductory lectures, General Psychology, Intersession, University of California, Berkeley, 1928. [Unpublished.]

attention, emotion, thought, action, and motivation; of adolescence, senility, woman, sex, music, art, literature, and invention, and so on *ad libitum*. So Psychology's plurality approaches infinity. It has its own instincts [2] and abstractions [3] and so also its uses and abuses!

When the then for-the-first-time 'new' Experimental Psychology was in the air in the '90's, introspection was only a method applied to the limited field of conscious content. Perhaps it never pretended to be a doctrine in and by itself. It was not until it had so asserted itself and become so formidable that behavior's *-ism* began to consciously define its contour. Functionalism also was eager to assume shape, while purposivism was forced to do so. And so figure and ground arrested our attention for some time. They were even reversible, now one as figure against the other as background for distinction, now *vice versa*. In Germany, the 'notorious varieties' of *Psychologie* are almost as numerous as there are *Psychologen*, and they are all 'closed,' 'articulated' figures against the background of the Wundtian tradition.

This *doctrinal* anarchy has made 'psychology' almost an index of personalities, no matter how distasteful psychologists feel at the very word personal. Practical investigators also demand *divisional* and *topical* dissociation, just as the morbid interest of the general public is revealed by such *caricatural* designations as 'magic and divination,' 'afternoon tea,' 'the market place,' etc. 'Psychology' has in time acquired these *doctrinal*, *personal*, *divisional*, *topical*, and even *caricatural* meanings. There is no longer *a* psychology. There are *Psychologies* in all these five different categories. They became so numerous that people began to label them by the calender at intervals of five years, '*Psychologies of 1925*,' '*Psychologies of 1930*,' and so on. Perhaps it is more fitting to speak of 'psychological sciences' rather than 'a psychology' or 'psychologies.' No student of science is more at a loss with his field than the student of Psychology with his so-called science.

[2] W. McDougall, The use and abuse of instinct in social psychology, *J. Abn. & Soc. Psychol.*, 1922, 16, 285–333.

[3] K. Dunlap, The use and abuse of abstractions in psychology, *Phil. Rev.*, 1927, 36, 462–487.

The Moving Picture Analogy

While divisional and topical categories demanded by practical need for separate investigation are not difficult to orientate, doctrinal and personal varieties of psychologies are extremely hard to reconcile, since each tends to capitalize certain patent concepts to its logical, factional, and possessional limit. Either one identifies himself with a certain particular school, or else one must take some attitude in regard to his own theoretical standpoint. Having neither the ambition to found a doctrine (for 'founding' nowadays everybody with an obsession can do, and it is no secret) nor the taste to affiliate myself with current ones (not even Eclecticism, for as soon as it assumes the dignity of a school not minding the suffix 'ism,' it ceases to be genuinely eclectic), I have been trying for several years to assume some sort of common-sense attitude or viewpoint in looking at things. At last, one little incident gave me the clue! It is cinematography, the *actual taking* of motion pictures.

The Function of Analogy.—As this common-sense viewpoint is entirely analogical, a word or two will perhaps be necessary for pointing out the importance and function of analogy. It can be empirically demonstrated that analogy is the essence of abstract thinking. If one proceeds in the usual test spirit by selecting some standard English dictionary, say, *Webster's International*, and by counting and sorting from A to Z the words that originally meant concrete objects, events, or situations, but either later exclusively, or at the same time metaphorically, signified the corresponding relations, similarities, or affiliations; one will find that language itself is diffusely permeated by analogies, which are mostly, perhaps, 95 percent, visual. A better and more direct procedure to the same effect would be to select a fair number of representative philosophical works by different authors in the different ages, to note one by one and page by page every illustration, every example, or instance that each author employs; and finally to realize that all authors resort to some sort of illustration, example, or instance, on the average, say, in every other page, or every three pages, that is, 30–50

percent as frequently as there are pages, and that these devices coming to their rescue consist of nothing but analogies that are in one form or another primarily visual.

Analogy is the anchorage (here and now I am using analogy) of abstract thinking, which usually cannot proceed very long without falling back upon some concrete, actual, or factual clue. It is definitely, though implicitly, employed by authors who may not give expressly the clue in point but who, especially if they are of the visual type, proceed nevertheless according to some sort of imagery as if translating what is in their mind's eye into verbal terms. In fact, when such be the suspected case, the very effectiveness and comprehensibility of an author's description is directly proportional to the ability that such description has for reanimating the assumed imagery; that is, its effectiveness is proportional to its vividness. The degree of intelligibility of a philosopher to the layman, other things being constant, increases proportionally as the frequency of analogies used by him increases. Perhaps the reason why concrete-minded persons have no patience in reading purely abstract writings is that, in the absence of analogical objects, situations, or events illustrating the ideas expressed, they lack somehow the initiative to look for these analogies themselves from the store of their own experiences. No relation or essence of things can be appreciated by anybody unless the things themselves are experienced. Given the relation or the essence of things without actually mentioning the things themselves, human understanding may be said to depend upon, or to consist of, the ability on the part of the understanding person to draw appropriate analogies.

The function of analogy in human understanding can be clearly shown by an analogy itself. Just as the memory image must have its temporal and spatial reference with a feeling of familiarity, so conceptual thought must refer to analogies for its setting. Just as a general image is evolved from a series of similar images into a single experience; so relations, similarities, or affiliations can be educted only from concrete objects, situations, or events. When these relations,

similarities, or affiliations arise in our mind spontaneously, as it seems, without some concrete setting for the time, we arrive at them suddenly with the feeling of newness. We believe we are in possession of a significant concept and thus begin to look back for concrete facts to develop our seemingly new idea. In other words, generalization comes first and then analogy. But strictly speaking, this is true only after our thoughts are put in black and white. In actual experience, it is always, I believe, concrete facts that suggest to us certain relations, similarities, or affiliations. It is perhaps a law that our nervous system retains better the relation of objects, the similarity of situations, and the affiliation of events rather than the objects, situations, or events themselves. We forget after a certain time all about the details of a certain face, a certain landscape, or a certain incident, but the 'general impression' of all of them lasts throughout our life. Common people ignore the concrete that leads to the general impression; artists, writers, and thinkers, while receiving a general impression perhaps not differently from others, observe the actual setting, emphasize the dominant feature, and by accentuating certain aspects of it, are able to communicate to others what actually arrested their attention. The artist depicts certain sentiments by an art object; writers bring out their characters in relief by relating certain trivial incidents; and so thinkers develop their ideas by analogies.

Let us go over some of the well-known analogies in psychology itself. We compare the eye with the camera, the organ of Corti with the piano, and the nervous system with the telephone or telegraph system. In each case, we 'picture' the unfamiliar mechanism in terms of some familiar mechanism. In other cases, we draw an analogy in order to diagrammatize certain relations of attributes, such as the color pyramid, the odor prism, or the tridimensional feeling coördinates. Thus consciousness is likened to a stream, to the field of vision with its levels of clearness, and to the figure and ground in perception; subconsciousness has a 'censor,' memory has a 'trace,' and above all mind is a 'sieve.' The

reflex mechanism is no more and no less than a slot-machine; body and mind are convexity and concavity, or as Warren has insisted,[4] surface and mass-weight. McDougall cannot present his 'drainage' theory intelligibly enough without resorting to the hydraulic analogy, which is used by others also for other purposes. Warren's only argument against the behaviorist's identifying motor expressions with central nervous operations is what he calls the 'thermometer fallacy.'[5] If the psychology of Horwicz is built upon the insecure foundation of analogies,[6] the for-the-third-time 'new' psychology of *Gestalt*[7] is based upon the same foundation, only a little less insecure.

An Incident.—The analogy that suggested to me a common-sense attitude or viewpoint toward doctrinal and personal psychologies is the *actual taking* of motion picture. This process is so familiar to everybody that, like all things that are too familiar, it escapes our finding in it anything of much significance. So I shall describe the incident as it actually occurred to me as if people know nothing about it.

Those who have visited Stanford University campus in California can easily recall the spatial orientation in the Inner Quadrangle, as one faces the Memorial Church with the Memorial Court at one's back adjoining the Quadrangle. For those who have never been in the place, the following topographical sketch (Fig. 1) will be of more help. The different buildings along the four sides of the Quadrangle and the Court, as in the entire 'Quad,' are connected by archways, which present a perspective that is unique when one views it from one end of the archway to the other. There are eight encircled places within the Quadrangle, within which there stands a circular bench among trees and flowers.

One fine sunny afternoon just after lunch in December, if I remember correctly, of 1928, I noticed, on my way back

[4] H. C. Warren, The mental and the physical, PSYCHOL. REV., 1914, 21, 79–100, p. 83.

[5] H. C. Warren, Psychology and the central nervous system, PSYCHOL. REV., 1921, 249–269, pp. 258–259.

[6] O. Klemm, History of Psychology, Eng. trans. from German, 1914, p. 217.

[7] W. Köhler, The new psychology and physics, *Yale Rev.*, 1930, 19, 560–576.

to the laboratory, as I drew near position 1, a group of three or four students there ready to 'shoot' with a moving-picture camera evidently at some scene among the trees and flowers. Closing up hurriedly to a position near the di-

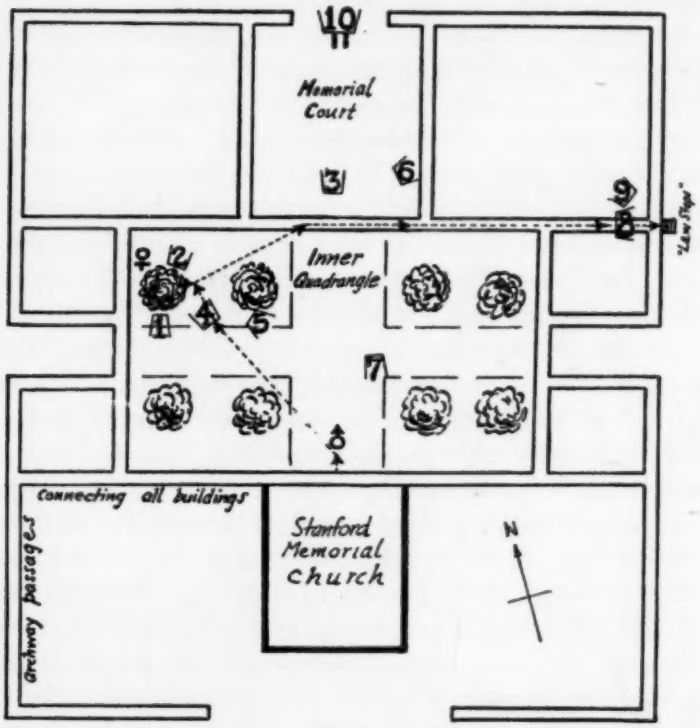

FIG. 1. Stanford University Campus Quad, showing the 10 successive positions of the moving-picture camera in photographing the meeting of a campus girl and a campus boy in the Inner Quadrangle to go to the Library.

rector and cameraman, I presently found out that it was a girl that they were aiming at. She was rehearsed to sit on the bench reading a book and then to stretch and yawn and finally to turn her eyes toward her left through the footpaths as if looking for somebody. I followed the director and cameraman to position 2, where they managed to shoot a few feet over her shoulder to catch a glimpse of the Freshman's 'Citizenship' that she was reading. Then the camera was directed, at position 3 in the Memorial Court, straight toward

the Church. A boy among the small group that came out of the Church, hurried toward the direction of the girl's place. The camera was shifted to position 4, also toward the Church, in front of which stood the thick trees. The boy coming out behind the dense short trees from our right was yelling and running toward us. We changed our position again to 5, where they photographed the girl's delight in hearing and seeing the boy, whom she met at last just outside the encircled short trees and beautiful flowers. Talking tête à tête they walked hand in hand toward the right of the camera's view. Some focusing was again done at position 6, where we watched them walking up into the archway between the pillars supporting the arches. We followed them further on at position 7 as they passed successively through the archway and behind the arches. Their figures grew bigger and bigger in the camera's field as they advanced toward it now stationed at position 8, where we got a unique perspective of dozens of archways extending backwards some hundred yards. I witnessed one last shot of the camera at position 9, where the girl was challenged to stop by a group of law students as she was descending the 'Law Steps' tabooed to Stanford girls according to tradition. The boy descended, but the girl had to be carried down. They went to the front of the Hoover War Library for some more posing and picture-taking. But I thought I had enough viewpoints of the camera; so I quitted them with a new idea.

The story is simple enough. A campus girl is waiting for a campus boy to go to the Library. Action consists of nothing but meeting, talking, and walking. The distance covered by the actors is only about a hundred yards or so. And yet it required no less than nine different positions of the camera to record this little story on a reel of film perhaps hundreds of feet long. I was struck by the fact and wondered how many positions they actually need for an ordinary picture plot of about two and a half hours' duration. I bought a sheep counter and began to count, during the next few months in the theater, the actual number of shiftings of the cameraman's position in each picture I saw. This, of course, de-

prived me of the appreciation of the story as a whole; and it was no easy job to watch the change of scenes either. The exact record is not at hand; but I remember vaguely that the total number of shiftings of the cameraman's position in each of about half a dozen pictures I counted was no less than 500–600. No audience ever bothers about this constant shifting of viewpoint; and when one takes notice of it, one is astonished to find that there are so many.

An Analogy.—The analogy I want to draw is now almost self-evident. The field of psychology, or rather the whole field of human knowledge, *as printed*, is one coherent plot in the picture *as determined* by the reel of film; so different categories of psychologies are different phases of the story. Each author who has anything of a doctrine, like the director and the cameraman at one time or another, assumes one and only one distinct viewpoint and these viewpoints are incompatible with one another. When books, monographs, and journals present the different views of the total field of science, as when the picture is shown in the theater, the majority of the general public, like every one in the audience, take the attitude of appreciation and see only the panorama of the story of things. Just as the different scenes as viewed from different positions of the camera are themselves discrete, so the different schools of psychology seem to be detached from one another; but usually the *audience* has no difficulty in connecting them up into a coherent story, and rarely do we need to demand everything *in print* in order to appreciate the interrelations of different authors and doctrines. The director of a picture, in order to ascertain a good effect for his cameraman from a certain viewpoint, may sometimes be forced to try a number of positions before reaching what he needs, though some of them may afterwards be taken for a later scene: so not infrequently an author of one school may actually creep into some other's point of view temporarily, not to give up his own position, but to learn how to contrast his own with the other's; he is not totally ignorant of the beauty of nature from perhaps all the viewpoints, but here and now for the sake of doctrine he is bound to return to his only

favorite position to claim at best that what is seen from other angles is equally seen from his; in other words, he ignores other viewpoints or else interprets them only in his own words from his own standpoint. Without pressing, however, too strenuously upon the closeness of our analogy, we may say that doctrine founders, like directors in the studio, cause the different aspects of our science to appear to be viewed by them from their respective master viewpoints under artificial illumination, instead of actually shifting their own position as directors must do in the case of outdoor scenes. The analogy can be pressed, nevertheless, one step further: authors may be the cameraman at different viewpoints at one time or another, so that they need not know the total story they are photographing; or authors may be compared with directors who, having a definite story to produce, assume a series of more or less closely related viewpoints concerning only the central theme of the story, but who know nothing about other directors' totally different series of viewpoints depicting radically different stories.

The fundamental principle of recording motion in a series of still pictures for reproduction in quick succession has been utilized perhaps hundreds of times by writers for various purposes. But so far as I am aware, nobody has seen anything of significance in the actual shifting of viewpoints of the camera in photographing a series of different scenes: indoors, outdoors, downwards from the sky, upwards under the ground, and in numerous other tricky ways of photographing, connecting up into a coherent story. One unit of action consists of a series of pictures on the film under one background, that is, taken from one point of view; an episode consists of several units of action by perhaps different actors under different backgrounds viewed from different standpoints; these episodes constitute a story. If the discrete movements of one unit action in quick succession produce a meaningful act, the equally discrete actions in an episode can give us one coherent story only by the fact that they are related parts of the whole. These discrete actions that come from perhaps different actors under different circumstances

(seen from different viewpoints) are really more discrete than we might usually think. In fact, when we first saw pictures without any previous training, we frequently wondered why the director took pains to show us a certain totally detached fragment of a scene in the midst of some continuous action. We did not realize the significance of it at that time. As we saw pictures more and more, we began to appreciate its relation to the whole story. In the written story these fragmentary incidents appear less abrupt than in the picture, because we find usually some verbal implication of its real meaning in the whole.

The distinction between the discrete, instantaneous pictures of one continuous action on the film and the even more discrete, abrupt shiftings of scenes of separate actions in the story can be best illustrated by the diagram in Fig. 2. The

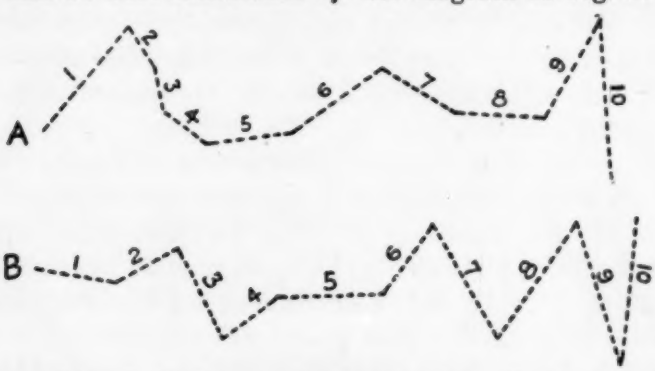

FIG. 2. Diagram showing two reels of moving-picture film *A* and *B*. The 10 broken sections represent 10 separate scenes taken with the camera stationed at 10 different places. The dashes making up the zigzag lines denote the discrete, instantaneous pictures of continuous movement recorded on the films. No two films have the same number and direction of camera viewpoints.

zigzag broken lines *A* and *B* represent two reels of film depicting two different stories. The entire film consists of thousands upon thousands of distinct pictures, indicated by the short dashes. But for a certain length of film, the series of pictures are taken of certain units of action under one background from a certain viewpoint; for the next series, they represent another scene from another viewpoint, and so on. Thus the whole line is not straight but in zigzag fashion,

showing that the story is made up of different episodes. The direction of any section of the zigzag lines denotes the viewpoint from which that series of homogeneous pictures is taken. When Bergson speaks of '*Le mécanisme cinématographique de la pensée*,' he is utilizing only the familiar fact that discrete, instantaneous pictures in quick succession produce movement; while I am here concerned particularly with the perhaps more significant fact that only by shifting abruptly our viewpoints toward various scenes of action can we really and practically account for '*Évolution Créatrice*.' [8]

Gestalt of Psychologies

We are now in the position to give a brief survey of the current schools of Psychologies. Science is a dynamic whole; its usual division into different branches is due to the limitations of human endeavor. Aside from the practical significance of these numerous divisions, the only justification for so doing is the difference of points of view. Psychology is a part of the hierarchy, and the prevalence of viewpoints in this branch of science, which is itself one specific viewpoint toward nature, is the most extensive and the most vigorous. From any one viewpoint, all other viewpoints are wrong: thus "the egotism of contempt for 'barbarians' and esteem only for 'Rome'" [9] of the numerous schools of Psychologies is a notorious, patent fact. Just as the cameraman can only focus his camera at any one given time toward a limited field of view in the total background, so *any one author can only pay attention to certain aspects of the total field of our science, if one really wants to build anything of a doctrine at all.* It is an intellectual necessity as well as a cinematographic one that *we should see things both spatially and temporally from different*

[8] Just as I was planning to write this article, I chanced to come across H. Bergson's Évolution créatrice, Trente-sixième Édition, 1930. The first line of the title of the last Chapter IV caught me, which reads '*Le mécanisme cinématographique de la pensée.*' In a footnote, he noted that the comparison was developed from 1900 to 1904 in his lectures in Collège de France, notably in a course on *l'Histoire de l'idée de temps* (1902–1903). The distinction noted here between my analogy and his comparison is significant.

[9] E. G. Boring, The *Gestalt* psychology and the *Gestalt* movement, *Amer. J. Psychol.*, 1930, 42, 308–315, p. 308.

angles in order to present the continuity of the whole. Let us list, in the following Table 1, the well-known current schools of Psychologies roughly into ten viewpoints corresponding to the ten positions of the camera as illustrated in Fig. 1, remembering, of course, that the closeness of the analogy cannot be rigidly pressed. Some of the schools are grouped together for convenience and should not be interpreted to mean that they are identical in viewpoints.

TABLE 1

VIEWPOINTS OF PSYCHOLOGIES

Viewpoints	*Common Designations*	*Leading Exponents*
1.	Entwicklungspsychologie	Krueger
2.	Introspectionism (Content Psychology)	Wundt, Titchener
3.	Act Psychology, Functionalism	Brentano, J. R. Angell
4.	Purposivism (Hormic Psychology)	McDougall
5.	Gestaltpsychologie (Strukturpsychologie), Organismic Psychology	Wertheimer, Koffka, Köhler, Bentley, Wheeler
6.	Persönliche Psychologie, Self Psychology, Eidetik typologische Psychologie	Stern, Calkins, Jaensch
7.	Geisteswissenschaftliche Psychologie, Verstehende Psychologie	Spranger, Jaspers
8.	Behaviorism	Watson, Weiss, Tolman, Hunter
9.	Freudianism (Psychoanalysis)	Freud, Jung, Adler
10.	Eclecticism, Dynamic Psychology, Reaction Psychology (Scientific Psychology)	Warren, Woodworth, Dunlap

The best way for an extremely brief approach to the various viewpoints in psychology is to follow the director and the cameraman, as I did in the above illustration, when they shift from one position to another in their earnest effort to present us an entertaining picture as a whole. When we share the viewpoint of Krueger in an *Entwicklungspsychologie*, we are reminded of the fact that mental life has a developmental history. Psychological facts need to be understood in this light. Everything has an origin, a development, before it reaches the phenomenon here and now. Not only in

perception but also in nearly every mental process, a mere following up of the course of events in its entirety gives us the complete explanation already in its better half. This is so in the psychoses, for example, where an effective diagnosis into the developmental etiology of prodromes constitutes almost the chief affair of prognosis. In the course of events of nature, however, some segment may remain completely hidden, thus making our intelligent understanding of a certain event frequently well-nigh impossible. Consequently we are forced to devise theories and hypotheses to hook up the missing link in a causal chain, so that a fairly rational explanation can be entertained, for the time being at least. But in the social and psychological realm, the discontinuity of phenomena is often not real but taken for granted, not inevitable but permitted, not destined by nature but decreed by human nature. There is always a censorship, social and psychological, intellectual and moral, to prevent the complete disclosure of phenomena. And this censorship is a hierarchy: among friendly circles it is intimacy; in public gatherings it is propriety, elegance, and taste; in the press it is censorship *par excellence*. Biography, cynical literature, and history of wars furnish abundant instances for this. The importance of the developmental point of view in psychology can hardly be exaggerated, because not only in the lower mental processes but especially in the higher sentiments, character, personality, and thought, the genetic approach gives us at least the faith in *complete* revelation of the natural course of phenomena. This is viewpoint No. 1. Analogically we have been watching the girl in our illustration *reading and waiting*.

Now we take a close-up view to discover what the girl is reading as we take viewpoint No. 2. *Introspectionism* has crystallized from Wundt to Titchener into that point of view where only the generalized, normal, human, adult mind yields introspective data of sensory elements and combinations as the contents (*Inhalte*) of consciousness. Such is the natural outcome, because, on the one hand, the specialized, abnormal, infrahuman, maturing minds had been *held by* others as independent fields possessing different viewpoints in opposition to

it, and because, on the other, it could not very well compass all the fields all at once with such magnifying and telescoping procedure, just as the cameraman cannot show us what the girl is reading unless he turns the field of view of his camera primarily toward the Freshman's 'Citizenship' book. Mental life would be as barren as the caveman's without differentiation. As a viewpoint, Introspectionism is significant because it is only by such a magnifying and telescoping procedure that we are lead to the finer nuances of the richness of mental life. As a method, writers as Warren, for example, use it in all psychological analyses [10] that yield results very much resembling the protocols from the laboratory, and I do not see why an introspective attitude should not be useful in everyday life.

However, the elementarism that resulted from Content Psychology tends to blind us to the psychological forest by attending only to the psychological trees. There are not only sensations but also acts of sensing; not only images but also acts of imaging; not only perceptions but also acts of perceiving; not only attention but also acts of attending. The *present participles* are just as real as, if not more palpable than, the corresponding *nouns*. It is ingenious perhaps to analyze attention into sensory clearness, but to equate the former with the latter involves a fallacy; we are attributing to the act a necessary content, whereas they are demonstrably separable in experience, independently variable, and characteristically different. Qualitative differences in acts do not necessarily correspond to qualitative differences in contents; neither is there a concomitant variation in intensity between the two, although they reveal a comparable temporal persistence. Finally according to Külpe, contents and acts obey different laws. Thus the *Act* school from Brentano onwards became early opposed to the *Content* school of psychologies. Acts occur as ideating, judging, loving, and hating. But these acts have functions. They are not isolated from the environment; they are successful coördinations to situations.

[10] H. C. Warren, A study of purpose, *J. of Phil. Psychol. & Sci. Methods*, 1916, 13, 5–26.

Adjustment and adaptation are primary facts of mental life: so *Functional Psychology* as incidentally expounded by Angell is neither an 'Is,' nor merely a 'Being,' but an 'Is-for' and a 'Being-for.' Mental operations have the 'How?' and 'Why?' aspects as well as the 'What?.' Consciousness has a utility. These are viewpoints No. 3.[11] We do not need to analogize in our cinematographic illustration.

McDougall chooses viewpoint No. 4. He sees the boy, as we remember, yelling and running toward him, and strives laboriously to convince the whole world that the boy is urged by the mating instinct, for example, toward the girl on whom McDougall's attention is focused. Purposive striving reveals itself in 'spontaneity of movement' with 'persistence of activity independently of the continuance of the impression which may have initiated it'; although there is 'variation of direction of persistent movements,' we see presently the 'coming to an end of the animal's movements as soon as they have brought about a particular kind of change in its situation' and immediately we have 'preparation for the new situation toward the production of which the action contributes'; we witness also 'some degree of improvement in the effectiveness of behavior; when it is repeated by the animal under similar circumstances': thus we see 'the totality of reaction of the organism' (seven marks of behavior).[12] This is *Purposive Psychology* in essence.

Gestalt Psychology is favored with viewpoint No. 5, where we are rewarded with the closing sight of the meeting of the boy and the girl. They are there for each other: neither of them alone has any social meaning for us from this standpoint. It is relations that we want to emphasize here. Sensory organization has the properties of organized wholes. Mental phenomena are to be explained by dynamics as opposed to machine theory. Subjective experiences have the characteristic marks of objective experiences in the sensory field of an observer; in other words, objective and subjective

[11] E. G. Boring, A history of experimental psychology, 1929, pp. 350, 446–447, 541–544.

[12] W. McDougall, Outline of psychology, 1923. Cf. Boring's History, p. 460.

experiences of the same behavior are essentially similar, so that we may use our objective experiences as a basis for inferring about the inner processes in others. There is no association without organization. The total field in our sensory experience includes the self and its attitudes; reproduction occurs only insofar as attitude coincides with the direction of existing associations. Insight is experienced determination, the feeling of the 'why's' and 'how's.' [13] Bentley's 'organization' is nearly *Gestalt.*[14] Wheeler's 'organismic psychology' is the whole of the organism.[15]

We hasten through the rest of the viewpoints. From standpoint No. 6, we have ample opportunity to compare individually the boy with the girl, their manner of walking, their facial expression, and, if we had a sound picture of them, their vocal expression in talking. Each reveals a somewhat different personality; each has a 'self,' so to speak. Hence, we have in Stern a *Persönliche Psychologie*, in Calkins a *Self Psychology*, and in Jaensch an *eidetik-typologische Psychologie.* Finding that they are behind the pillars of the archways and sometimes hidden from the view of the passers-by in the Quadrangle and the Court, the boy may have stolen a kiss from the girl as is suspected from their mutual head positions when they enter the next archway. Viewpoint No. 7 can catch up such an interpretative scene as this. *Geisteswissenschaftliche* or *Verstehende Psychologie* as represented by Spranger and Jaspers has much that appeals to us, if not experimentally sufficient to convince us. Of course, *Behaviorism* favors viewpoint No. 8, where Watson can simply admire the stream of behavior of the young couple, walking in the perspective of the archways and subvocalizing at the same time; where Weiss can identify the continuum of human movement with the perspective continuum of the dozens of archways about it, so that the two are but electron-proton aggregates; and where Tolman and Hunter, unwilling to give up the simplicity and uniformity, can simplify and unify

[13] W. Köhler, Gestalt psychology, 1929.
[14] M. Bentley, The field of psychology, 1924.
[15] R. H. Wheeler, The science of psychology, 1929.

purpose and consciousness. We are sorry to depict an embarrassing scene in viewpoint No. 9, because the girl has deliberately forgotten all about the taboo of the 'Law Steps' and unconsciously stepped on them only to be mocked at and later to be carried down by the boy. *Freudianism* has much psychology to overcome in order to gain acceptance from the academic circle, but *Psychoanalysis* and Jung's *Psychology of Types* and Adler's *Individualpsychologie* are certainly psychological viewpoints just as dignified as any others.

So far we have been following the cameraman and the director from viewpoint to viewpoint to see positively what we have to see from each and every standpoint. But to employ an analogy within an analogy, we are liable to lose our *psychological forest of viewpoints* by attending only to our psychological trees. So let us imagine that we erect a tower at the central position 10 in the Memorial Court (Fig. 1) and, supposing there are no buildings to obstruct our view (as is not actually shown in the diagram), that we mount the tower to give our audience a concluding panorama of the entire nine scenes from beginning to end, when the boy and girl are asked to react the episodes continuously this time. Warren's viewpoint is nearest to this position than any others. Our director persuades us that such a panorama is interesting and so the cameraman gives us one last viewpoint No. 10.[16] Woodworth and Dunlap have not quite succeeded in giving us a complete panorama in explicit terms, although they mounted the tower themselves only to satisfy themselves.

Let us see what can be distinctively characteristic of this last panoramic *Eclecticism* of Psychologies. In the first place, by virtue of the high panoramic position, one is able to take into account the total stimulation-adjustment-response series of events. There are three distinct methods of psychological research; self-observation, behavior, and the neurological. A psychology from this standpoint can be developed, as Warren has done, from elements to organization with much informal introspection of objective behavior into conscious operations and mental states, which act and function with forethought

[16] H. C. Warren, Human psychology, 1919.

and anticipatory reaction, the interaction between the organism and the environment being the selective and reactive transfer of energy patterns with intermediate processes of every sort. A practical delimitation of the field of psychology,[17] while not defining completely any single point of view of the different schools, can be deliberately formulated so as to include all the psychological activities of all professional psychologists. In the second place, this Eclecticism can be self-consistent, self-contained, and self-sufficient. Warren, for example, starts with the fundamental assumption that the universe is a unitary system, and, always guided by the Canon of Parsimony, proceeds to build his system from bottom to top in as mechanized a fashion as the pyramid was built. The hierarchy of sensation, perception, imagery, feeling, conation, emotion, sentiment, volition, and thought is extremely rigid, elementaristic, and exacting. Although always practically parallelistic in treatment of nearly all mental facts in the most thorough manner, he insists on the identity hypothesis or double-aspect view to avoid the eternal metaphysical question. In the third place, this Eclecticism can be ultimately neurological as psychology should be. One may think of every and all psychological phenomena in terms of neurology. One may picture every process, every operation, and every mental state as representable, describable, and visualizable by its neurological correlates. One could reduce everything back to the senses, for example, purpose back to the distance-receptors, as indeed Warren has done. Thought is, after all, perhaps only nervous current draining and rushing from center to center but somehow not necessarily leaking out of the cortex. In a word, Herrick's characterization of psychology is capable of serious elaboration.[18]

In a way this unique Eclecticism is a *Gestalt*, to use a psychological analogy, of all Psychologies. One could easily insert all the positive aspects of all schools, as any *genuine Gestalt* psychologist should do, into this systematic structure

[17] H. C. Warren, Outline of a psychological standpoint, *Amer. J. Psychol.*, 1927, 39 (Washburn Commemorative Volume), 23–41.

[18] C. J. Herrick, The thinking machine, 1929.

and tolerate every point of view insofar as it is constructively illuminating. Just as in our cinematographic illustration, this viewpoint enables us to catch at least a superficial glimpse of all the scenes and happenings that are sights and insights from all the rest of the viewpoints. We are relatively safe in a position where we can view our science as a whole in perspective and '*gestaltize*' *the totality of the fields of psychology* as '*Gestaltists*' *self-assertedly insist* more and more *in* psychology. In this broad but impossible sense, any one taking this point of view may be caricatured as a genuine *Gestalt* psychologist. Shall we, then, adopt this Eclecticism? But wait!

Conclusion

In summary, psychologies fall into five categories: *doctrinal, personal, divisional, topical,* and even *caricatural.* Doctrinal and personal varieties of psychologies themselves need psychological orientation. The best way to this end is by analogy, which is universally present in theoretical writings and decidedly helpful for better human understanding. The analogy that suggested to me a common-sense attitude or viewpoint toward this problem is cinematography, that is, specifically the *actual taking* of motion pictures of the story of a campus girl waiting for a campus boy to go to the library. Different psychological viewpoints may be compared with the different positions of the director and cameraman, where a limited field of view is obtainable only one at a time. Thus the magnifying and telescoping procedure of *Introspectionism* for discovering mental elements is like taking a close-up view over the sitting girl's shoulder to reveal the fact that it was a Freshman's 'Citizenship' book she is reading. *Purposive Psychology* happens to occupy a position so favorable for observing the characteristic marks of the boy's running and yelling behavior, that McDougall labors to convince us that the boy is urged, for example, by a mating instinct toward the girl, who is just at the back of McDougall and of whose presence he is aware without turning around. *Gestalt Psychology* is delighted to see from another position that the boy and the girl are a *couple,* no longer *a* boy and *a* girl; although

Behaviorism, choosing a symmetrical position at the end of a long passage of archways, notices later on the simplicity of their walking up the archway and the uniformity of the arches themselves in perspective, and so does not hesitate to identify the two in terms of electron and proton aggregates. *Freudianism* presents us with an embarrassing view from another angle when it depicts the scene of the girl unconsciously stepping on the campus 'Law Steps' that are tabooed to her sex by tradition.

None of the viewpoints tells the whole story. Fortunately Warren's position can give us an *Eclecticism* of Psychologies which we should take special pains to build by erecting a tower in some suitable position so as to have a panoramic view of the whole scene, from the girl's reading and waiting to her unconscious stepping on the forbidden steps. I have pointed out that this possible, unique Eclecticism may be called the *Gestalt of Psychologies* but have hesitated to adopt this naïvely. Why? Because it is still *a* viewpoint! And because a true *Gestalt* or a genuine Eclecticism cannot be insisted upon!

We have been following so far only the director and cameraman shifting from position to position to photograph the different scenes including the last hypothetical one in panoramic perspective. We have not yet actually witnessed the showing of the picture on the screen. Well, we go to the 'show' or, as the Europeans say, the 'cinema,' and sit *in the audience* to wait for the operator in the booth upstairs to reproduce the whole story on the silver screen. At last, we get our money's worth—our evening's entertainment. The majority of us do, but some in boxes, some in loges, some in stalls, and some in the balcony. So none of us theater-goers gets even the *hypothetical unitary viewpoint of the 'audience.'* But we must not complain. Only a picked few can be actors, directors, cameraman, and operators, although if it were not for us *in the audience*, all of them would be out of a job.

Similarly we go to lectures, books, journals, and pamphlets for psychological news and views from lecturers, authors, and editors, that is to say, *the majority of us do;* although some are

natured and nurtured to found doctrines and theories. Shall we *insist, in turn, on an audience point of view?* Shall we, then, propound a *Gestaltism of Psychologies?* No, if everybody remains in the audience, whence comes the show? Studio, actors, director, cameraman, operator, *and* audience constitute life, and life is one. Psychologies and Psychologists constitute *a Gestalt of Gestalten, a super-Gestalt.* I have no intention to abuse the concept of *Gestalt,* but I see already the *reductio ad absurdum* of *Gestaltism* when I hammer it to its logical conclusion. When the fanatic public is led only to the relation and whole of things, they forget all about the rôle of the parts of things, just as the *audience* are forever blind to the fact that, while they are enjoying the *story,* they never realize the discrete, abrupt shiftings of *viewpoints* of the camera.

[MS. received October 2, 1930]

DISCUSSION

THE AVOIDANCE OF REPETITIVE RESPONSES

Thorndike (PSYCHOL. REV., 1927, 34, p. 234) has presented data to show that when words or nonsense syllables were read to subjects at a uniform rate and it was required that they write a number (0 to 9 or 1 to 9) whenever they heard a stimulus, they generally avoided repetitions, *i.e.*, such sequences as 00, 11, 22, etc. In the first experiment different words were read to 6 subjects at the rate of 2½ sec. per word. While by chance one would expect 10 percent of repetitive sequences, the actual percentages obtained were 2.9, 3.3, 3.3, 3.4, 3.9 and 9.1 for the 6 individuals. A second less extensive experiment with 8 subjects resulted in similar findings. In a third experiment with nonsense syllables read at the rate of 5 sec. per syllable the tendency to avoid repetitions was not so clear. The percentages obtained from 8 subjects were 2.0, 5.1, 6.3, 7.8, 7.8, 11.1, 12.0 and 12.8 respectively as against 10 percent by chance.

The report was made with the suggestion that the data might bear upon Dodge's refractory period theory of learning and of behavior variations in general. In comment Dodge (PSYCHOL. REV., 1927, 34, p. 237) stated that "it is not difficult to imagine a crucial extension of Professor Thorndike's experiment. If various time intervals between stimuli could be used, a suppositious refractory phase in the barrier against repetition should theoretically be more apparent the nearer the responses approach each other in time."

Aside from the refractory phase theory Professor Dodge also mentioned several other plausible hypotheses which might account for the experimental results but which he rejected. For instance, the subjects might have a conscious or unconscious bias to distribute their associative responses or this might be just the habit of the group. The main reason for excluding such hypotheses was supposedly the unanimity and consistency of Thorndike's results. Exceptions, however, become clear when one presents the data in percentage form as above.

I shall now report a further attempt to throw light on the relation between the avoidance of repetitions and the refractory phase.

The experiment performed was very similar to Professor Thorndike's with the following modifications in procedure.

1. Four different time intervals were used, 1 sec., 2 sec., 4 sec. and 6 sec. Shorter intervals proved impractical. The purpose for this variation has been suggested in Dodge's statement.

2. We used three kinds of stimuli. (1) Taps on the table at regular intervals of 1, 2, 4, and 6 sec. (2) Taps presented irregularly, the four intervals appearing in a random manner, no interval being repeated more than twice in immediate succession. (3) Letters of the English alphabet at regular intervals. Taps of more or less uniform intensity are presumably more similar to physiological stimuli ordinarily used in refractory phase investigations than either words or nonsense syllables. Responses to such monotonous taps can be compared with responses to more variegated stimuli,—in our case, letters. Irregular taps were introduced as a means to interfere with any bias or mental set that the subjects might have fallen into and also as a medium grade of variegated stimuli. As a matter of fact the two kinds of taps produced about the same results.

3. In treating the data forward sequences such as 12, 23, etc. and backward sequences such as 98, 87, etc. were also checked. It was argued that if the subjects should show characteristic tendencies toward such habitual sequences as well, then the avoidance of repetitions could be better understood in a general setting.

The stimuli were presented in groups of 51 each. Eight such groups could be given in an experimental period, with short rest intervals between. Only one kind of stimuli was used in any single experimental period, *i.e.*, regular taps, irregular taps or letters. The time intervals for any 8 consecutive groups of regular taps or letters also appeared in a chance order. The experiment covered 6 periods so that each subject returned only 200 sequences for each of the three kinds of stimuli and the four lengths of intervals, altogether 2400. Only numbers from 1 to 9 were written. Six subjects served throughout the experiment, all advanced students in psychology. On account of practice effect one of the experimental sessions had to be repeated for one of the subjects and two for two others. Otherwise the data returned on one day agreed very closely with subsequent days.

The following table summarizes all the results. The tendency to avoid repetitions is very clear in 5 of the 6 subjects. With the other subject the reverse is true. So his data can only be tabulated

separately in parenthesis. However, all the subjects took advantage of the habitual sequences forward and backward. The figures represent the percentages of repetitive, forward and backward sequences out of the total number of responses made. By chance there should be 11.1 percent of repetitive sequences and 9.9 percent of forward or backward.

	Regular taps		Irregular taps		Letters	
	For 5 S's	For 1 S	For 5 S's	For 1 S	For 5 S's	For 1 S
Repetitive						
1 sec.	5.9	(34.5)	9.8	(33.0)	2.2	(8.0)
2 sec.	4.3	(30.0)	5.7	(24.5)	3.1	(14.5)
4 sec.	5.1	(19.5)	2.8	(16.5)	3.2	(15.5)
6 sec.	5.2	(21.0)	3.7	(15.0)	3.5	(16.5)
Forward						
1 sec.	21.8		20.1		30.3	
2 sec.	16.9		18.0		17.0	
4 sec.	15.8		15.3		15.9	
6 sec.	13.9		15.8		14.3	
Backward						
1 sec.	17.2		19.6		16.8	
2 sec.	15.3		17.7		16.2	
4 sec.	14.1		14.0		13.3	
6 sec.	15.0		13.9		13.1	

Retrospective reports were taken at the end of the experiment. No subject guessed the purpose of the experiment right. Two reported that when a particular number had appeared too many times, that number was avoided. Another subject (not the one whose data appear under parenthesis) testified that whenever in difficulty he resorted to the figure 1. Four of them had one or more numbers consciously favored. One tried to avoid forward sequences sometimes, another backward sequences, still another both.

Conclusions

1. There is a tendency to avoid repetitions in 5 of the 6 subjects but there is no such tendency for forward or backward sequences. On the contrary, these habitual sequences are taken advantage of while avoiding repetitions, the forward sequence to a larger extent than the backward.

2. The responses to letters give the lowest percentage of repetitive sequences while the responses to regular taps give probably the highest percentage. As to forward sequences exactly the reverse is true.

3. The three kinds of sequences all decrease in number with increase in the time interval. There is, however, a very significant exception. With letters as stimuli the number of repetitive responses increases regularly with the time interval. The table also gives the lowest percentages in this particular case. If this positive correlation between the length of the time interval and the number of repetitive responses can be demonstrated in future experiments of a more extended nature, an explanation is to be sought for. The conditions for this particular section of the experiment resembled most closely Professor Thorndike's.

These conclusions suggest that the refractory phase in its strict physiological sense cannot be a plausible explanation for the avoidance of repetitions as we observe in these experiments and in everyday life. It is possible, of course, that even in very complex human behavior the elementary condition of variation, as Professor Dodge put it, can be traced down to the telescoping of refractory periods. Our experiment failed to give the crucial test as he expected. The shortest time interval used was 1 sec. At the end of that interval both the refractory and the rebounding phenomena must have been events of the past. It may be practical to further shorten the interval with a different kind of responses required. But simple physiological conditions cannot be reproduced in any such experiment as ours.

I am rather inclined to think that the common avoidance of repetitions is due to one or another of the causes which Dodge supposed to be unlikely. There is, for instance, a bias, more conscious than not, in at least two of the subjects to distribute their responses. The very instruction, 'Write *any* number, (or *a* number), etc.,' might have led to such a mental set. Quite naturally it would be worked out in the line of least resistance by following habitual sequences such as the forward and backward.

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